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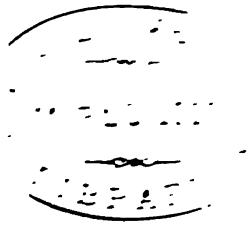

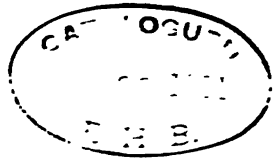

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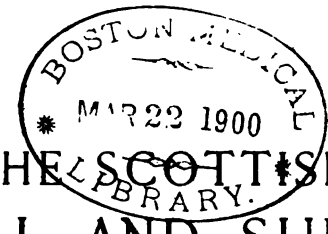

THE SCOTTISH MEDICAL AND SURGICAL JOURNAL

EDITORIAL NOTE

AT the beginning of a new year and of a new volume of the *Scottish Medical and Surgical Journal*, our friends and readers will be interested to know how the Journal fares. It will be remembered that three years ago the Journal was promoted by a number of medical men, who contributed liberally to a guarantee fund, and whose object it was to have a journal which would represent the medical work of Scotland. During the past three years the Journal has been accorded a gratifying measure of support, and it is being steadily sought for by Societies and Libraries which profess to have the leading Journals on their reading tables. While our success has been gratifying we are still far from being satisfied, for, in view of the Journal being so distinctively Scottish, we ought to have a larger body of subscribers in Scotland than we yet have.

With the end of the third year of the existence of the Journal it became desirable to put it upon a permanent commercial footing, and with that object in view the original promoters converted themselves into a Limited Liability Company, with a capital of £2000, the shares in which were readily taken up by the profession.

The Journal is henceforth to be published by the Company.

The staff has been strengthened by the addition of Dr Norman Walker as sub-editor, and the general management is under his care in association with the editor Dr Russell.

Some alterations have been made which it is expected will make the Journal more attractive. The colour of the cover has been changed and the size of paper has been increased. Modifications have been made in connection with Digests and Abstracts with a view to making them still more useful.

We hope that our friends will continue to interest themselves in adding to our list of subscribers, and we assure them that every effort will be made to maintain and enhance the position the Journal has already made for itself.

It will be a convenience if readers would note that all business communications should be addressed to the Sub-editor, 7 Manor Place, Edinburgh, and all editorial communications to the Editor, 3 Walker Street, Edinburgh.

NOTES ON THE TREATMENT OF DISEASES OF THE NERVOUS SYSTEM

By J. J. GRAHAM BROWN, M.D., F.R.C.P.Ed.,

Assistant Physician to the Royal Infirmary; Lecturer on the Practice of Medicine, School of Medicine of the Royal Colleges, Edinburgh

Lecture III.—Treatment of Peripheral Paralysis, or that which results from affection of the lower motor neuron [1]

SUMMARY

The lower motor neuron—diseases which affect it—symptoms thereby produced—reaction of degeneration—its value in estimating the probable results of treatment.

The treatment of peripheral paralysis—removal of cause—necessity of rest—local bleeding—application of heat—counter-irritation—use of drugs.

Electricity—the objects for which it is to be used—its power of restoring conduction—experiments of Remak—and of Heidenhain.

Effects of galvanic currents—electrotonic action—catalytic action—electrolytic action—cataphoric action.

How the galvanic current should be applied—results which follow its use—conditions under which improvement or cure may be expected to result—unfavourable indications.

Stimulating effects of electricity—why these are useful—how they are obtained—method of procedure—conditions under which stimulation of nerve and muscle is of service.

Massage—its advantages—objects to be served—cases in which it is especially useful—methods.

Exercise—voluntary movements—opposing resistance—gymnastics—danger of over-fatigue.

Contracture—how this is to be avoided.

As you are aware, the lower motor neuron consists of the nerve-cell lying in the anterior cornu of the spinal cord, or

[1] Delivered in the Royal Infirmary.

in the corresponding medullary or cerebral nuclei, with its protoplasm, its nucleus, and the arborisation of its dendrites, of its axis-cylinder process or axon, prolonged to become the efferent nerve fibre, and of its terminal arborisation connected with the substance of the muscular fibre which it innervates.

Diseased processes may strike at the whole or at any part of this neuron, and may be of very varying character. Any lesion involving the anterior horns of the cord (or the corresponding motor cerebral nuclei) which destroys the cell, destroys at the same time the whole neuron, because the cell is its trophic centre, and produces for a like reason rapid atrophy in the muscle. Or it may be that the axon running in the peripheral nerve may alone be affected, the result usually of pressure, wounding, or other local cause. Or, finally, the neuron may be attacked as a whole, by the toxines of disease, by the products of faulty metabolism, or by some metallic, alcoholic, or other extraneous poison.

But whether it be the neuron as a whole which is attacked, or the cell in the cord, or only the motor nerve in its course, the result as to motor symptoms is very similar. And these motor phenomena, familiar to you from the study of many cases during this winter, are briefly as follows:—

The muscles innervated by the affected neuron are removed from the influence of the will: they are paralysed. And, owing to the fact that their trophic centres lie in the cells of the lower neuron, the muscles rapidly atrophy, and for a like reason they lose their tonus and become flaccid. The reflex loop being destroyed by the lesion in the neuron the deep reflexes are lost. And, finally, there is present that curious phenomenon first described by Erb, the reaction of degeneration. This reaction, highly important in regard to diagnosis and a valuable guide to prognosis, is also useful, as you will presently see, in relation to treatment. Stated briefly, its features when fully developed are as follows. The nerve when stimulated by the galvanic or by the faradic current gives no response, *i.e.* no muscular contraction follows. Nor does any contraction result from the application of a faradic stimulus over the muscle itself. But when the muscle is stimulated by the galvanic current it contracts, often indeed it responds in this way to a current too weak to give rise to

contraction under normal conditions. And the muscular contractions so produced are sluggish, and not sharp and almost instantaneous as in health. Also you will frequently find that the anodal closing contraction is more readily elicited than the cathodal, a condition opposite to that which obtains in health. In fact the polar conditions are often so altered that we get, in order of current strength :—

(1) A.C.C.
(2) K.C.C.
(3) K.O.C.
(4) A.O.C.

instead of the normal—

(1) K.C.C.
(2) A.C.C.
(3) A.O.C.
(4) K.O.C

But while these may be taken as the characteristics of the reaction of degeneration, and the most important of these is the sluggishness of the response to galvanism, a lighter and less fully developed form of the reaction is often met with in which the faradic irritability, though diminished, is not completely lost, and in which the nerve still conveys a galvanic stimulus. You will presently see the importance from the point of view of treatment of recognising this lighter form of the reaction of degeneration.

Turning now to the consideration of the treatment of these forms of disease, I have first to tell you that, in many cases, paralysis of the lower neuron is very amenable to treatment. You will sometimes get results which are surprisingly good, more so, I think, than in any other form of nervous disease.

In the first place, then, having made your diagnosis, and having satisfied yourself that the lesion with which you are dealing is one of the lower motor neuron, it is your next duty to ascertain, as nearly as may be, the cause which has acted in the individual case you are studying. In many cases, indeed in the large majority, you will be able to do this with a fair approach to accuracy. If the neuron is affected as a whole, you will look for evidence of the presence of toxines of infective disease, of altered metabolism and auto-intoxication as in

diabetes, of metallic poisons such as lead, arsenic, mercury, or of alcohol. If, on the contrary, the lesion is one acting on the trunk of the nerve, you will search for such traumatic causes of neuritis as pressure, tearing, wounding, and the like.

And, if you are so successful as to discover it, the first step in treatment must necessarily be an attempt to remove that cause. Now I have, in a former lecture, spoken in some detail of the means by which the removal of the toxines of disease and other poisons should be attempted, or by which their deleterious action may be combated. To this we need not now return. In this connection, however, a word may be said as to the treatment of acute anterior poliomyelitis during the febrile stage. We possess, unfortunately, no antitoxine for this complaint, though it may be that the future may bring us one, and therefore all we can do towards eliminating the poison is to see that the bowels are kept well open, preferably by means of calomel, and that a somewhat profuse diaphoresis is at once established. In addition to this, the administration of salicin has by some observers been found useful.

In traumatic cases where the nerve has suffered from compression or tearing, the result of fracture of bone, of dislocation, tumour, inflammation of neighbouring parts, or the like, surgical aid is called for. But even if it be possible to remove the cause of the paralysis by surgical measures, the injured nerve is not necessarily thereby restored to health, and much careful treatment will usually be found needful before its function is regained. It is hardly necessary to say that in the case of a wound severing a nerve, the cut ends should be united as soon after the injury as possible.

The importance of rest in treatment of these paralyses of the lower neuron is very obvious. The patient should be confined to bed until all acute symptoms have completely disappeared. For any movement involves some attempt at least to set up functional activity in the neuron, and this is not desirable so long as an acute process is going on. In acute poliomyelitis, for example, it would be very harmful in the early stage to stimulate those nerve-cells which are within the diseased area, and in that affection, as well as in multiple neuritis, a single exertion may do great damage to the cells and fibres implicated. But not only is it very harmful to attempt to set

in activity these diseased structures during the acute stage of the affection, but in multiple neuritis as well as in traumatic neuritis the contraction of the muscles through or between which the affected nerves pass may produce such compression as may be very detrimental to recovery. For these and for other similar reasons confinement to bed should always be enjoined, and the patient be instructed to keep the affected parts as still as possible.

The abstraction of blood by means of leeches or wet cupping is sometimes of considerable service at the outset in cases of rheumatic facial paralysis and other local inflammatory lesions of peripheral nerves. In the same class of cases hot poultices and fomentations applied over the inflamed region of the nerve have a very soothing effect, and probably mitigate the severity of the process. You will also find that hot poultices applied to the spine over the seat of the lesion are very grateful to the child suffering from acute poliomyelitis. In cases of local inflammatory neuritis when the acute stage has passed away it is sometimes advisable to apply fly blisters, or even the stronger counter-irritation of the cautery over the seat of the lesion. But if you are going to apply a blister in such a case, be careful not to do so at a point where there is anæsthesia, for in all probability the nutrition of the skin will be so lowered in that area that you may have great difficulty in getting the blister to heal. In multiple neuritis these external applications are not nearly so useful as in those conditions of which we have been speaking.

The treatment by drugs is not very successful in cases of paralysis of the lower neuron. In all instances tonic treatment with iron, cod-liver oil, strychnine, quinine,[1] etc., will be required in the later stages. But it is difficult to obtain any very decided medicinal action on the progress of the disease itself. Iodide of potassium, sometimes useful in the chronic stages of multiple neuritis, is undoubtedly of service in localised inflammation of a nerve trunk, and its utility is increased even in non-syphilitic cases by combining with it small doses of mer-

[1] The use of arsenic as a tonic in these cases is very questionable, because it is itself capable of giving rise to neuritis, even in comparatively small doses. If it is employed, its action should be watched with great care. For the same reason the doses of mercury given should be small.

cury. You will find salicylate of soda of use in cases of rheumatic neuritis, and it should be tried in every case of lesion of the lower neuron where the attack seems to have been brought on by exposure, and especially when there is any suspicion of a rheumatic tendency in the patient. Antipyrine is also of service under these conditions. I have seen several cases in which small doses of phosphorus appeared to do considerable good.

Strychnine is undoubtedly of great use in some cases of progressive muscular atrophy. Sir William Gowers, who relies upon it in this affection, holds that it arrests the disease in more than one half of the cases. It should be given hypodermically, and the dose, at first small ($\frac{1}{60}$ grain of the nitrate of strychnine), should be rapidly increased to the limit of tolerance. The dose should be administered once a day, and the treatment continued for many months. Strychnine sometimes appears to do good in acute poliomyelitis. In such cases it should be given by the mouth and in very small doses, and great care should be taken not to begin its administration until six weeks or two months have elapsed since the commencement of the disease.

The cardiac weakness which accompanies some forms of multiple neuritis, particularly that due to diphtheria, may require the use of special remedies, alcohol, digitalis, strychnine, camphor, musk, etc.

Electricity is one of the most important therapeutic means at our command in cases of paralysis of the peripheral neuron, and you have had many opportunities of observing and of estimating the benefits which follow its employment.

For the moment we shall exclude from consideration the action of electricity on the sensory nerves to which our attention will be subsequently directed, and discuss only its therapeutic action on the lower motor neuron and upon the muscles which that neuron innervates.

The whole subject is exceedingly difficult and obscure, resting as it does in part on exact experiment, in part on empiricism, but I think you may take it that there are, speaking generally, two main ways in which electricity may be usefully employed in such cases as those we are now considering. In the first place, where a motor nerve no longer transmits

voluntary impulses, you may endeavour to improve its nutrition and restore its function, and for this purpose you will use the galvanic current in the manner to be presently described. In the second place you may have to stimulate to contraction those muscles which have been cut off from their trophic centres in the cord and which are consequently wasting. For this purpose you may sometimes employ the galvanic current, but you will find that the faradic current is more generally of service.

Dealing now with the first of these methods, I may remind you of the experiments of E. Remak which go to show that the passage of a galvanic current through a motor nerve produces an increase of excitability in the nerve so treated, an excitability which persists for some time after the current has ceased to pass. You also remember the often quoted experiment of Heidenhain, by which he showed that when a muscle-nerve preparation has been so damaged that it will no longer respond to the opening or to the closing of a comparatively strong current, you are able, provided the damage has not been too great, to restore its function by passing through it a galvanic current in an ascending direction, the current being continued for a longer or shorter time. Of the accuracy of these and many similar results there cannot be the slightest question. When you come, however, to consider the clinical aspects of the matter you will not find things so clear.

The good results of the application of a galvanic current to a motor nerve which is no longer conducting voluntary impulses consist partly in its electrotonic action; that is, in those obscure changes which show themselves in an increase of excitability at the negative pole and a decrease at the positive. They are also due to that combination of effects called by the elder Remak the catalytic action of the current. This term—not to my mind a good one, but commonly employed—may be taken to include the electrolytic action, that molecular dissociation and rearrangement which takes place in the tissues at and between the poles, as well as the cataphoric action of the current by which an osmotic flow is set up in the fluids of the tissues in the direction from the positive to the negative pole. To this may be added the action of the current on the blood-

vessels supplying the nerve, whether that action be on the fibres of the vascular wall or on the vasomotor nerves.

In these and probably in other ways the nutrition and activity of function of a motor nerve may be deeply affected by a galvanic current, and there cannot be any reasonable doubt that most favourable results are produced by this form of electrical treatment in many cases of peripheral paralysis.

These results on the nerve, which we have been considering, are obtained, you will observe, during the passage of the galvanic current, and are to be carefully distinguished from the stimulating and exciting action which takes place when the current is made or is broken. You should indeed, for the purpose we now have in view, avoid as far as possible these stimulating effects, and therefore it is best, if you can make the arrangement, to put on the current after the electrodes have been applied by means of a rheostat, so that it may be slowly and steadily increased to the amount you require, and at the end of the sitting as slowly and steadily cut off. In this manner any rapid oscillations are avoided. The current employed should not as a rule exceed 6 milliampères (indeed 2 to 3 m.a. may often be sufficient), working with electrodes measuring from one to two inches in diameter. The length of time during which the current is allowed to pass should be about five minutes, more or less according to the acuteness of the case, and the application may be made daily or every second day. You will find it best to place the kathode or negative pole as nearly as may be over the injured part of the nerve, while the anode is applied to an indifferent point such as the sternum. It is hardly necessary to remind you that the electrodes should be thoroughly soaked in warm water before being applied to the skin. You should watch the galvanometer very carefully during the application of the current, for the resistance of the skin rapidly diminishes as the current passes, and it may become necessary to diminish the current strength in consequence.

Using the galvanic current in this stable way, you may expect to see some immediate improvement in favourable cases. During its application the patient will probably experience some slight return of voluntary motor power, the conducting power of the nerve having been more or less

restored. If the lesion is recent and the case a favourable one, you may obtain complete restoration of motility after three or four applications of the current. In other cases it may require twenty sittings or more before so favourable a result is reached, and in any case the cure will be accelerated in its later stages by making use of the stimulating power of the galvanic or of the faradic current in the manner to be presently described. It is needful, however, that you should fully understand that these good results—and they are sometimes brilliant in their success—are only attainable in the lighter cases, when the lesion lies in the course of the nerve, and when the reaction of degeneration is not well marked. If the fibres of the neuron are actively degenerated, you will not obtain any such distinct success. But even in these it is well to apply the galvanic current occasionally, say once every four or five days, for it is exceedingly likely (though there is no absolute proof of this) that the current exerts a favourable influence on the process of regeneration of nerve fibre. The earlier a case comes under treatment the more successful that treatment is likely to be, but even in cases of some standing the results may be good.

From what has been said it will be clear to you that this form of treatment is useful in cases of paralysis produced by localised lesion of peripheral nerves. In the so-called rheumatic facial paralysis most successful results may be obtained, especially if the case be seen early. The kathode (about one inch in diameter) should be applied to the mastoid or over the point of exit of the nerve, the anode being placed on the sternum, and a current of from 1 to 3 milliampères applied. The sitting should not as a rule exceed five minutes in duration.

Lesions of the nerves of the limbs are also, as a rule, very amenable to this form of treatment, indeed it was in connection with radial paralysis that E. Remak[1] published those statistical results which have enabled us to appreciate so distinctly the value of galvanic treatment in cases of paralysis of the lower motor neuron. In such cases the

[1] E. Remak, "Ueber die antiparalytische Wirkung der Electrotherapie bei Drucklahmungen des N. radialis."—*Deutsche Zeit. f. Nervenheilkunde*, Bd. iv. (1893), S. 377.

electrodes should be two to three inches in diameter. The kathode should be placed over the seat of the lesion, the anode over the sternum, and a current of from 4 to 6 m.a. should be employed.

From what has been already said you will gather that when the paralysis depends on parenchymatous degeneration of the nerve fibre, in toxine poisoning, in multiple neuritis, no such successful results are to be looked for. Indeed in cases of this form of neuritis it is not advisable to employ electricity during the early part of the acute stage. But when that stage has begun to subside the galvanic current may be usefully applied, provided that a rheostat be used and that the opening and closing shocks be carefully avoided. Currents of 2 to 3 m.a. with electrodes of about two inches in diameter are sufficient, and the anode may preferably be applied over any tender nerve trunks. Considerable good may in this way be effected and the process of regeneration hastened.

Still less may we expect success from this form of treatment in those cases in which the lesion lies more particularly in the anterior horns of the cord. It is sometimes used in infantile paralysis, but I do not think that in that disease or in the more chronic forms of poliomyelitis or in progressive muscular atrophy you will produce any distinctly beneficial result. Perhaps an exception to this may be found in the case of bulbar paralysis, in which disease the stable application of the galvanic current occasionally does some good. The electrodes, of considerable size, should be applied over the two mastoid processes, and a current of 1 to 3 m.a. employed. It is particularly important to make use of a rheostat in these cases, and to avoid all rapid variations of current.

Turning now to the other form of electrical treatment in peripheral paralysis—that, namely, which depends on stimulation of nerve and muscle—I have first of all to tell you that, while this is of great service in many cases, it may be safely used only when the acute stage has completely passed away. A certain amount of such stimulation may be brought about by the so-called labile use of the galvanic current, that, namely, in which, while one electrode is kept stationary, the other is slowly moved over the nerves and muscles. For this purpose

a roller electrode may be employed. The movement of this from point to point together with the fact that the area of the electrode in contact with the skin varies somewhat according to the compressibility of the tissue on which it happens to rest at the moment renders the current unsteady in amount and this produces a stimulating action on nerve and muscle.

A more distinctly stimulating effect is however produced by making and breaking the galvanic current or by means of faradism, or by combining both forms of current and muscular contraction may be brought about either by placing both electrodes directly over the muscle or by stimulating the nerve at its motor point.

The muscular contraction so occasioned, when produced in muscles which are largely or entirely incapable of voluntary movement, is very useful in keeping up their nutrition and tone. The blood flow in the contracting muscle is accelerated, and the more mechanical pressure exerted on neighbouring veins and lymph channels tends to prevent stagnation in these vessels.

But, in addition to this, the faradic current appreciably affects the conductivity of the motor nerve through which it passes. If you stimulate such a nerve with a current a little weaker than is required to cause muscular contraction, you will find that after the current has been passing for a little time the excitability of the nerve has been so increased that muscular contraction commences. And it is found by experiment that this excitability persists for some time afterwards. But you must not suppose that by the strongest faradic stimulation of a motor nerve above the seat of lesion you can break through the barrier which is opposing motor conduction. That is not possible, and to attempt to do it forcibly may be very harmful.

And there is yet another point which you must not forget, namely, that by the stimulation of the sensory nerves which the faradic current 'particularly the faradic brush' produces it is capable of reflexly affecting the motor fibres. For, in the case of diseased processes in mixed peripheral nerves you have to remember that as a general rule the motor fibres suffer most, and that it is usual to find that, though no efferent impulses can pass down the nerve, efferent sensory impressions are still con-

veyed upwards. The powerful stimulus of the faradic brush transmitted in this way to the centres gives rise to centrifugal impulses down the motor fibres, and although these cannot break through the obstacle to motor conduction if the paralysis is complete, yet they are of great benefit, when regeneration is in progress, in completing the cure in cases of peripheral paralysis.

In stimulating the muscles to contract, in these peripheral cases, by means of the faradic current, you should bear in mind that you may readily produce considerable exhaustion. Physiological experiment has shown that the exhaustion of motor nerves when faradised increases with the frequency of the interruptions, and you will therefore arrange your apparatus so that the making and breaking in the primary coil are not too rapidly effected. Once a second is about the rate at which I like to see the hammer vibrate, and you may find that an even slower rhythm is sometimes advisable. As to the strength of the current to be employed, you may take it that what will give a distinct and good muscular contraction is sufficient. More than this is not necessary and, indeed, very strong stimulation may be distinctly harmful.

In neuritis resulting from inflammation of or pressure on the nerve trunk, when the chronic stage is reached, much good may be done by means of electrical stimulation. The galvanic current with slow interruptions may be used, the strength being just sufficient to cause muscular contraction, and it should be remembered that if the reaction of degeneration be well marked it may be necessary to use the anode in order to obtain such contraction. If the muscles can be made to contract with faradism, that form of current may also be employed. I have seen much benefit result from faradic treatment even in inveterate cases. But you should lay it down to yourselves as an absolute rule that such stimulation is not to be used if any pain remain after its application, and, further, that on the appearance of any contracture all stimulating electrical treatment should at once be stopped.

In multiple neuritis, when the acute stage has completely passed away, galvanic stimulation of nerves and muscles may usefully be employed, and, somewhat later in the case, the

faradic current may be made use of. But here also care should be taken that no after-pain results.

In acute anterior poliomyelitis no electrical treatment will do good in the case of those cells and fibres which have undergone complete degeneration, nor in that of the muscles the trophic centres of which are thus destroyed. But there are always cells and nerve fibres in which, though their function is affected, the process has not gone its full length. In these and in the corresponding muscles partial if not complete recovery is still possible under suitable treatment. For such nerves and muscles electrical stimulation is of considerable value, but it should not be commenced before the third week. It is best to employ galvanism in the first instance, indeed you may find in many cases that it is only by galvanism that the muscles can be made to contract. Later in the case faradism does well. But, in as much as these cases occur in children, you have to be very careful in so using electricity as not to frighten your patient, and it is necessary to commence with very weak currents, or indeed to use no current at all, until the child has become accustomed to the application of the electrodes. The strength of current may then be gradually increased, but it should be at no time greater in amount than that just necessary to cause distinct muscular contraction.

In progressive muscular atrophy, on the other hand, very little good is effected by the use of electricity; but, nevertheless, it is right to give that form of treatment a trial in every case, using faradism if the muscles can thereby be made to contract, or if not, galvanism. But you should see that the current employed in either case is a weak one, for undoubtedly strong stimulation does harm in this disease. In bulbar paralysis some little benefit may follow faradisation of the lips, tongue and palate, but here also the currents used should be very weak.

Too much prominence can hardly be given to the benefits which result from massage in cases of paralysis of the lower motor neuron. When rightly conducted the nutrition of the tissues, and particularly of the muscles, is greatly improved by this means of treatment, and the return of venous blood and of lymph accelerated. How much such stimulus to blood-flow is required is shown by the blueish tinge and the low temperature of the paralysed limb.

In all forms of neuritis and acute poliomyelitis, massage is very useful. In no case, however, should it be employed until the acute stage is well over, and in neuritis care should be taken that the rubbing is not made over inflamed nerves. The treatment should be begun gently, light friction being applied in the first instance. Later on in the case kneading movements may be made use of, which, in cases of traumatic and pressure neuritis may ultimately be performed with the full force of the operator. I have repeatedly seen great good result in this affection from massage which was distinctly painful. In multiple neuritis and in poliomyelitis any rubbing or kneading which causes pain should be avoided.

In the first instance the affected limb should be stroked with the flat of the hand—always in an upward direction—over its whole surface. Then, grasping the affected muscles with the whole hand or with both hands, but not with the fingers only, the operator should knead and roll these muscles thoroughly. Going over the whole of the affected muscles methodically, a quarter of an hour or half an hour may thus be spent, but fatigue on the part of the patient is to be avoided. The process should be repeated at least once a day, and to this treatment bathing the limb with hot water containing sea-salt may usefully be added.

In this way the nutrition of the muscles which have suffered may be greatly increased, and in some cases they may even be brought back to a normal condition. There remains, however, for notice the most powerful stimulus of all and the most natural, namely, Exercise. When the muscles which have been paralysed begin to recover the power of voluntary movement, when the motor fibres begin again to conduct impulses from the cortex, such voluntary movement should be encouraged. As the days pass you may add to the usefulness of such voluntary movements in promoting the return of power by opposing resistance with your hand to such movements, a resistance which should be gradually increased from day to day. A little later in the case gymnastic exercises may be attempted, the variety prescribed depending, of course, on the group of muscles which you desire to have exercised. It is in such cases that the various forms of Zander's apparatus are so use-

ful. But in all these voluntary exercises the very greatest care should be taken not to fatigue the patient.

In all severe cases of neuritis and in poliomyelitis you have to guard against the occurrence of contracture. In the so-called rheumatic facial paralysis it is very apt to show itself. If it does so, all electrical treatment should at once be given up and massage substituted. The affected muscles should be rubbed between fingers placed inside the mouth and outside on the cheek. The use of a wooden ball held inside the mouth, over which the muscles may be rubbed, is recommended by some. In cases of multiple neuritis it is very important that the feet be not kept continuously in an extended position. The pressure of the bed clothes, which favours this, should be removed by means of a cradle, and the feet should be kept at right angles to the legs by a properly adapted sand-bag or by light splints. In poliomyelitis the same precautions have to be taken if the foot is affected, and the child should not be allowed to lie for long with the knee-joints flexed.

MUNICIPAL DUTY AS TO THE PREVENTION OF TUBERCULOSIS[1]

By JOHN C. M'VAIL, M.D., D.P.H.Cantab.,
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THE subject of the discussion which I am asked to open to-night has been selected by the Society's committee, and the selection is indicative of the importance which is now attached to the prevention of tuberculosis. The interest taken in the question is not surprising. A quarter of a century ago consumption was looked on, popularly and even professionally, as a physical curse, passing on from one generation to another, sometimes loosening, but never letting go its hereditary grip of the families which, in some unknown way, had come within the wide sphere of its evil influence. Nothing could be relied on to prevent attack, and the science of medicine could, as a rule, only prolong life and palliate suffering. For the afflicted families the outlook was indeed gloomy, and it had become all the

[1] Paper read in opening a discussion at a meeting of the Civic Society of Glasgow on November 23rd, 1899.

darker with the advance of civilisation and the increased appreciation of the value of human life, and the freer play of natural affection which had accompanied the lessening of the keenness of the struggle for the mere necessaries of existence. Into this gloom Koch's discovery of the tubercle bacillus came as a ray of light some seventeen years ago. The public, indeed, did not fully appreciate its significance; but it raised high hopes in the breasts of medical men, and only two years later a congress of the Sanitary Institute, held in Glasgow, accepted, almost as a matter of course, the view that the whole subject of tuberculosis had even then been brought fairly within the domain of preventive medicine. Later came the sanguine expectation of an inoculative cure for phthisis, followed by a natural reaction of disappointment. And now, at length, we have the whole civilised world deeply interested, to some extent even engaged, in the prevention of tuberculosis. This activity is in very great measure—in by far the greatest measure—genuinely philanthropic; but in addition there is probably at work another, and in so far as it is based on fact, a perfectly legitimate influence—the desire to avoid infection. For the same discovery which indicated how exaggerated had been our views as to hereditary agency, suggested what till then, so far as the public of this country is concerned, was an almost unthought-of danger—tubercular attack by direct entrance into the system of a specific infective organism.

It is outside the scope of this paper to discuss the bacteriological aspects of the subject. It may be taken that direct transmission of the tubercle bacillus from one generation to another is so rare as to be almost negligible, and that heredity so far as it is concerned has to do with susceptibility to invasion of the disease. I shall assume that the bacillus is the *causa causans*, and that it may find entrance to the body by means of human sputa, and by the flesh and milk of diseased cows. As to statistics, it seems necessary just to mention a few principal figures. In England and Wales tubercular diseases, including the maladies popularly known as consumption of the lungs, water in the head, consumption of the bowels, and scrofula, caused one death in every nine that occurred in the years 1891-95. It caused

as many deaths as the total group of principal zymotic diseases. Between the ages of five and fifty-five, 25 per cent. of all deaths were due to pulmonary consumption, and between the ages of twenty-five and thirty-five nearly one-half of all deaths were due to tubercle. Enormous as this mortality is, it was much greater forty years ago. Since the decennium 1851-60 the diminution in the tubercular group of diseases has been 39 per cent., and in the particular form of pulmonary consumption, over 45 per cent. In other forms, therefore, the diminution has been less, and, if we take *tabes mesenterica* as representing abdominal phthisis, it is found that at all ages the reduction here has been only 8 per cent.; under five years it has been only 3 per cent., while under one year there has been an increase of fully 27 per cent. It is probable, however, that the term *tabes mesenterica* has been somewhat loosely used in death certification, and the most recent evidence tends to show that in children consumption of the bowels is less prevalent, relatively to consumption of the lungs, than has usually been supposed. In Glasgow, I learn from Dr Chalmers's valuable paper on the "Distribution of Tubercular Diseases" that in the seven years, 1890-1896, in the Sick Children's Hospital, 24 per cent. of the admissions and 35 per cent. of the deaths were due to tubercle; that in 1871, in the city, the phthisis deaths formed 13·5 per cent. of all deaths, and that by 1892 the percentage had fallen to 9·6. In Manchester the phthisis rate fell from 2420 per million of population in 1881-85 to 2090 in 1891-95. In Sheffield the rate fell from 3059 in 1851-60 to 1521 in 1891-95. I give these figures not as exceptional, but because the records happen to be at hand.

Following naturally on such statements, the question of prevention is approached through the inquiry, What has caused the great diminution in tubercular mortality that has already taken place? The answer is a very satisfactory one. In some small measure, it is true, the improvement may be only apparent, and may be attributable to altered nomenclature, as a result of more accurate diagnosis. With this comparatively trifling exception, the decline is to be taken as due to better conditions of living. That is a wide statement. Improved house accommodation is probably the main factor in the change. Drier sites; damp-proof courses in walls; houses

raised higher above the ground level; better ventilation; the restriction of bedrooms without fireplaces and chimneys; open iron bedsteads in place of shut-in and curtained recesses; smooth wall papers easily dusted and washed; carpets not reaching to the wall, but convenient for lifting and cleaning, or small rugs instead of carpets, and closely boarded polished floors; more frequent and systematic refuse removal; greater personal and domestic and municipal cleanliness—all these are among the influences which have been at work. It is obvious that they operate in two directions. The same fresh air which is inimical to the life of the tubercle bacillus strengthens the lungs and the whole human body to resist its invasion. Good general health is, indeed, much more protective against tubercle than against any ordinary infectious disease. In typhus, enteric fever, scarlet fever, measles, and the like, admission of the virus into the system is far likelier to be followed by onset of the disease than where consumption is concerned. The material of tubercular infection is so abundantly present that no human being could expect to escape if susceptibility to it were as great as to ordinary infections.

From all this it appears that municipalities have already, long before they knew it, been actively engaged in the prevention of tuberculosis, and that already their labours have been very largely rewarded. It also appears that continuance of work on the present lines will meet with a continuing reward. The knowledge that measures of general sanitation, as applied to streets, and dwellings, and schools, and factories, and workshops, are very directly fruitful in diminishing the prevalence of phthisis, ought to be, and no doubt will be, a very considerable stimulus to health authorities everywhere, and especially in large towns where tubercular maladies are most to be dreaded. It is not for one moment to be supposed, however, that a great and active municipality will, in view of all that is now known regarding tubercular disease, be satisfied even with the maintenance of an already high standard of sanitary well-doing. The question for discussion to-night is, whether some special advance should be made along any existing lines of work, and whether there should be any new departure in any special directions. In the time at my dis-

posal for opening the discussion I propose very briefly to suggest for your consideration certain of the most important of these, without, of course, making any pretension to exhausting the list.

(1) All who had the pleasure of hearing the brilliant address given by Professor William Ramsay, of University College, to the Sanitary Congress which met at Gilmorehill three or four years ago, should have no difficulty as to one available preventive measure which may be pushed further than at present. I refer to the remarkable influence of sunlight in the destruction of the tubercle bacillus, and I doubt if there are many places which get so little benefit as the towns in the west of Scotland from this agency. What between our grey northern sky, our high rainfall, and the smoky Scotch coal which is our only fuel, we are at a great natural disadvantage in this respect, while the character of the prevailing industries greatly adds to the disadvantage. While every corporation should give reasonable heed to the interests of manufacturers, perseverance and activity in the detection and prosecution of offenders in the matter of smoke production is a manifest duty. There is constant talk of the harassment to which great industries are subjected under Factory and Workshops Acts and Rivers Pollution Prevention Acts, and the smoke abatement clauses of Police and Health Acts. But when necessity for improvement arises through enforcement of the law, it is really astonishing how problems which appeared impossible of solution are solved, and how the utilisation of waste products tends to repay outlays which had been incurred under prophecy of financial disaster, and of the encroachments of foreign competitors who, it is always alleged, are subject to no comparable restrictions. Factory and workshop chimneys, however, are not the only offenders. Domestic smoke comes to a large total of atmospheric pollution, though the individual items are so trifling, and I do not see why large cities should not ask for a considerable measure of control which could be used judiciously in accordance with advancing knowledge of methods of smoke prevention. It is hardly necessary to point out how the prevention of tubercle by this particular means coincides with what is and ought to be municipal policy, altogether independently of tubercle.

(2) If I mistake not, it was Dr Russell who was the first to lay special emphasis on the practical applications of the fact that so long as the microbes of infectious disease are kept in a liquid medium (other than drinking water or liquid food, such as milk), they remain perfectly harmless. It is mainly when carried by the air, or by direct contact, that danger is to be feared from ordinary infectious disease. In phthisis the infection is conveyed not by the breath of the patient but by the sputum, and (with exceptions which need not be detailed here) only by the dried sputum, borne by the air as dust. As a result partly of gradual improvement in public manners, and partly of bye-laws for public conveyances, spitting in cars and streets and public places will, no doubt, gradually diminish. But, in the meantime, can no definite step be taken in the matter? Think of the amount of sputum discharged on the pavements of popular working-class parades on Saturday and Sunday evenings in our large towns—say, in that long stretch of thoroughfare which runs east and west between Parkhead and Whiteinch—I had almost said from Tollcross to Dalmuir. In dry weather, even a Monday morning washing of the principal pavements would make a very appreciable diminution of the infective material to which the public is exposed. Of course, in the west of Scotland the abundant rainfall would sometimes render any such measure unnecessary.

(3) Without waiting to dilate on the question, I feel bound to mention under a separate heading the great importance of air space and ventilation in elementary schools. In the nature of things the minimum quantity of air space per head demanded even for a common lodging-house can hardly be asked for in a school where classes have to be concentrated for purposes of teaching. This renders ventilation all the more important, but it is impossible to expect natural ventilation to be able to cope with the difficulty. Artificial or mechanical ventilation should be the rule here, and in such very large schools as are now being erected in towns, the cost need not be prohibitive.

(4) The measures already mentioned have to do mainly with the prevention of infection from human sources. We come now to the bovine disease. Of the necessity for regular meat inspection by qualified veterinary surgeons in every city

there can be no manner of doubt, and the way here is so clear and unmistakable that only some temporary municipal perversity or unreadiness, or the evil influence of use and wont, can delay due progression along the proper road. In this matter the United Kingdom is very far behind continental countries. In Berlin there are no less than forty veterinary surgeons occupied in the meat inspection service. So far, however, as concerns the recognised sale of tuberculous flesh for human consumption (after careful sterilisation, of course), I do not think we are ever likely to follow the Berlin example. Sentiment plays a large part in the acceptance or rejection of food, and our working classes are fortunately in a much better position than Berlin artisans to give heed to sentiment. About two-thirds of the taxpayers of the German capital have an income not exceeding £45 a year, and the majority of two-roomed houses are rented at from £10 to £12, 10s. a year. With such an income and such a rent a man with a family to keep cannot afford to be very fastidious in the matter of butcher meat. With us it is hardly likely that meat, even rendered thoroughly safe by treatment, would find a ready sale if labelled tuberculous.

(5) Notwithstanding a well-known decision regarding tuberculous meat given in the Glasgow Sheriff Court a few years ago, the cow is dangerous much more as a milk giver than as a meat giver, and the risk from milk is especially great in infants and children, though as has been already indicated, there may have been some tendency to exaggerate this risk. It would be out of the question to occupy time here discussing the tuberculin test, and the separation or destruction of infected cattle, and the exclusion from milk byres of cows with diseased udders. These are subjects more for rural than municipal administration. Within the last few years there has been considerable improvement in the housing of cattle and in the condition of dairies. That improvement is going rapidly onwards. Bye-laws are being made more stringent and, what is of even more consequence, are being more stringently enforced; and under a new Order and circular by the Local Government Board there will be a further advance, especially as the whole community is becoming very acutely aware of the vital importance of the subject.

But while all this education and improvement are in progress, the serious fact remains that children continue to be infected by the very food which is intended to nourish them, and that so infected, they are dying themselves, after having aided in spreading the disease throughout the country. Can no immediate and definite, even if only temporary, action be taken to check or prevent this loss of life? Is there no short way with tuberculous milk? Every educated man and woman knows that sterilised milk is safe, but even among the better classes many families continue in the old way, and expose their children to the old risks. What, moreover, is known about sterilisation in the lowest classes, and how much or how little is done even in the artisan class? These queries suggest a further question, on which I should be specially glad to hear the views of the members of the Society. The question is, Should great municipalities ask legislative powers to prevent the unrestricted sale of raw milk within their bounds? Would this be an undue infringement of the liberty of the subject? Some folk say they cannot drink boiled milk. No doubt boiling even for a single minute, as recommended by the Royal Commission, is the safest way, but it is not the only way, as a more prolonged exposure to a lower temperature (170 deg. Fah.) gives a very useful result. Milk so treated is less unpleasant to the palate than boiled milk, but it could be arranged that, say under medical certificate, raw milk be supplied to objectors, from special dairies specially supervised. If anything were to be done in the way of compulsory sterilisation, it would have to apply not merely to milk produced within municipal areas, but to all supplies from the outside. Legislation might proceed somewhat on the lines of the Food and Drugs Acts. For a temperature under 180 deg. the testing would probably have to be bacteriological, but the analytical chemists, Messrs Tatlock and Thomson, inform me that chemical tests exist by which milk that has been heated to 180 deg. Fah. or more can be distinguished from raw milk. A medical man, before whom I put this proposal of compulsory sterilisation, and who has paid special attention to infant feeding, would go even further in the way of compulsion. Where infants are not breast-fed, he would insist on humanised milk being used. The process here is so simple that any working man's wife

should be able to learn it at a single lesson. A thirty-ounce bottle of milk is allowed to stand for two hours in a room at the ordinary temperature of about 60 deg. Fah. The effect is that the creamy part rises towards the surface, and the curdy part sinks. By means of a rubber tube the lower half of the contents of the bottle is syphoned off, an ounce of sugar of milk is added, the bottle is filled up with water, and the process is complete. Babies fed on such milk are said to thrive amazingly.

My own hesitation in making the suggestion of compulsion, even as to sterilisation, is due mainly to the fear lest such an enactment should, in spite of all efforts to the contrary, be taken as an excuse for delaying the much-needed reform of the milk production of the country. There should, if possible, be no departure from the high doctrine of the public health. We should hold no parley and enter into no truce with insanitation. Boiled tubercle may be better than raw tubercle, but ought an armistice or compromise or terms of any kind to be made with such an enemy? On the other hand, when a house is on fire, your immediate purpose is to save the inmates, and you don't stand idly looking on because you are aware that the Dean of Guild Court is taking more and more cognisance of structural arrangements for the prevention of fires. Or, to steal a differently applied illustration, when you see a drowning man in the water, you want to throw him a life-belt instead of delivering to him and the crowd on the pier-head a lecture on the art of swimming.

Such are the two sides of the argument as they present themselves to me. But all through the debate I seem to hear the cry of the children for immediate salvation from an immediate danger, and in presence of their pleading the ear is apt to be deaf to other considerations.

Short of compulsory sterilisation, a very useful procedure, which, I believe, has already engaged the attention of Glasgow, would be to search systematically for the tubercle bacillus in the milk supply, and wherever it is found, to prosecute under the provisions of the Public Health Acts regarding the sale of unwholesome food.

(6) The municipal provision of sanatoria for consumption is a question already ripe to be dealt with. In the evolution

of many well recognised public duties, both local and national, there has been a transition period in which private benevolence has held the place to be ultimately taken by systematic general assessment. In this matter of sanatoria we are passing through that stage, but, looking to the progress of public opinion, it is likely to be a short one, and sanatoria for consumption, it is safe to prophesy, will very soon be viewed, equally with fever hospitals, as mere matters of course in the way of local rating. In saying this, I do not for a moment suggest compulsory removal to sanatoria, nor their erection on a scale so extensive as to receive all or nearly all the cases that occur. As has been recently pointed out, the isolation of any given number of suitable cases of phthisis means an equivalent diminution in the risks of spread of the disease. Where scarlet fever is concerned, there is little use in sending one case to hospital from a tenement building and leaving another at home. Not so with phthisis. Each case removed is a definite gain. Very large schemes need not be entered on at first. Accommodation can easily be extended as the result of experience, and so also experience will soon give guidance as to the amount of municipal provision required for different stages of the disease.

It may be pointed out with reference both to ordinary hospitals for infectious disease and to sanatoria for consumption, that no one who respects his own reputation for common sense raises the question whether they are in accordance with the high doctrine of the public health. Yet if we depart from the basis of direct utility and deal only in theoretical considerations, it is open to hold that every hospital and sanatorium is a standing monument of sanitary failure. If we could achieve perfect success in the prevention of infectious disease by fresh air and cleanliness and the ordinary weapons of sanitary warfare, we would need no hospitals. But to propose to abolish them on that account would be to preach transcendental sanitation. After all, we are thus forced back to the question of utility in a pretty direct form. The use of the knife is theoretically the opprobrium of surgery, yet some of the greatest triumphs of surgery are attained by its means, and in the same way though hospitals and sanatoria may be considered the opprobria of sanitation, yet they are so

immediately beneficial that the provision of them is one of the most important duties of sanitary authorities.

(7) It has been frequently suggested, chiefly by medical men, that notification of the existence of tubercle in human beings should be made compulsory, as in the case of ordinary infectious diseases like scarlet fever and enteric fever. To that proposal perhaps the commonest objection has been that the accompaniment of notification would be isolation in hospital. That objection is groundless. Systematic notification would undoubtedly be useful, independently of removal to hospital. If the infected house were open to visitation by the public health officials, unsatisfactory conditions of overcrowding, want of ventilation or cleanliness, and so forth, could be dealt with, disinfection could be carried out when needed, especially in the event of a change of occupancy, and a register of infected houses and streets and localities could be kept, which might throw much light on the natural history of the disease. But there are difficulties in the way of compulsory notification which seem to me to make it impracticable at the present time. The first duty of a medical man is to his patient, and by some patients the intimation of phthisis would be looked on as a death warrant. The fear of the disease is still such that the mental anxiety and depression consequent on the announcement might often have a bad effect on the prospects of recovery. As consumption comes to be less dreaded, this objection will lessen in weight. The malady is indeed already less feared than of old, so far as its fatality is concerned. The difficulty could, no doubt, be got rid of by a proviso that the sanitary authority, as a distinguished Edinburgh physician has suggested, should take no action unless with the consent of the medical man notifying the case, but it is questionable if many local bodies would care to pay fees under such limitations. Then again, the public fear of phthisis as an infectious disease is now so general that a person known to be suffering from it would be under serious social disabilities. We have no right to create a leper class in the community, and to attach to individuals a physical stigma, which might not merely put them under great personal disadvantages, but might deprive them of the opportunity of earning a livelihood for wife and family, almost as effectually

as if they had to wear a leper's bell, or adopt the leper cry of "unclean." Many a man with a bad lung is yet quite capable of leading a useful life, and of following his occupation in factory or workshop. All he has to attend to is the sputum, but it might be very difficult to persuade his neighbour working at the same bench that no infection could be carried from the breath or perspiration, or by contact. So long as these difficulties lie in the way of compulsory notification, I would be glad to see a system of voluntary notification introduced, the medical attendant only intimating cases to which no such objections would attach. He ought, of course, to be properly remunerated for such notifications.

(8) As having a very direct bearing on the question of tuberculosis prevention, I desire to repeat and amplify some remarks which I ventured to make at the close of Mr Bosanquet's very interesting and suggestive address delivered at last month's meeting of this Society. But, in the first place, if someone asks me why I have not dealt with county rather than municipal duty in this matter, the reply partly is that this is the Civic Society of Glasgow, and that my subject has been chosen for me. County sanitary administration, besides, is barely ten years old, and District Committees are still struggling with elementary problems of water, and drainage, and scavenging and hospitals, which have already been solved in most cities. The town's need, moreover, is greater than the country's. Tubercle and zymotic diseases, and, indeed, nearly all diseases, are plants native rather to urban than to rural soil. Modern sanitation is itself born of the circumstances of city life. Long ago in London the death-rate was regularly higher than the birth-rate, and the conditions of Glasgow sixty and seventy years since were such as to counteract in some measure the natural increase, not only of its own population, but of the surrounding country's, which it constantly absorbed. An old medical man, who had been apprentice in the late twenties to a Dr Dick, whose surgery was in the Gallowgate, close to the Cross of Glasgow, used to tell me how at that time many tailors' workshops consisted of damp unventilated cellars, and how, time after time, workmen came fresh and healthy to such places from the country, and how they were cut down one after the other by consumption.

In a civilised community, so living and so dying, public health was a mere question of evolution. But so inferior are the efforts of man to those of nature, that not all that has been done these seventy years has lowered the city's death-rate to that of the country's. We have not yet succeeded in producing a *rus in urbe.* The one governing factor here is density of population. All the skilled labours of professional sanitarians, and all the self-denying work of local authorities earnest for the public weal, have been insufficient to overcome this primary difficulty in the way of a city's health. What we need is room to live, and one of the most hopeful things for the future health of this country is the extension and cheapening of travelling facilities, by which even the comparatively poor workers in our great towns can establish their homes and rear their families outside the central zones of smoke and fog and foul air.

Meanwhile, though it is impossible to veto the erection of huge tenements even in the country, they still have the fresh air around them ; and though it does occasionally happen that the rush from the city takes place too suddenly for the convenience of rural local authorities, yet these bodies have not been unwilling to learn and apply those sanitary lessons which have been originally evolved from the hard-earned experience of life in thickly-populated centres. The result is that to the abundant air space, which is the one supreme physical advantage of country life, there are added the benefits of systematic water supply and good drainage and dry dwellings built under rigid sanitary supervision. In this way, though a *rus in urbe* has not been realised, an *urbs in rure* is in some measure becoming an accomplished fact. Such cities-in-the-country will, in themselves, be sanatoria, in which consumption will be difficult to set agoing, and easy to check. Their indoor atmosphere may be as pure as, or purer than, the outdoor atmosphere of great towns, while children brought up in such surroundings will incline to lead that active open-air life which is the heart and soul of modern methods of curing tubercular disease. While, therefore, it is urgently necessary for municipalities to give attention to all the various specific measures which may be taken to diminish the prevalence of tuberculosis amongst the population living within their borders, perhaps the

most useful policy they can follow is, as Mr Bosanquet and others have urged, to promote and cheapen travelling facilities between town and country, to push their tramways far along the main lines of communication, to encourage railways, both light and heavy, to maintain roads in good condition for cycling, and, in short, to do their utmost to diminish the density of population within their own boundaries, and to make use of the great surplus air space which nature has provided all around them, and which is, and must always be, the surest preventive and remedy for every form of tubercular disease.

These are all the points to which I wish to call your attention, but it strikes me that in the recesses of some one's mind there may lurk the query *cui bono*, in regard to all efforts to prevent tuberculosis. Once, when a health officer reported to his sanitary board that in a given period the births had been 100 and the deaths only 50, his statement was met by an appalled listener with the exclamation, "God bless me, gentlemen, that's very serious!" That critic was, unwittingly, a Malthusian. But independently of, or perhaps even in opposition to, the doctrines of Malthus, the question is sometimes asked, Is not modern sanitation a policy of suicide, considered racially, however protective it may be as regards the individual? Are we not simply propagating a weakly race by coddling into manhood and womanhood puny infants and children, who would far better be allowed to die before they become fathers and mothers? In replying to such a querist let me at once say that I am not competent and do not propose to attempt to discuss the thorny question of the transmissibility of acquired traits. It does not, however, seem necessary to fall back here on the fundamental position that a medical man must do his utmost to save every life committed to his care, no matter how valueless or dangerous the preserved life may be. Nor does it even seem necessary, independently of professional duty, to mention the inborn love of parent for child, which would cast to the winds all such cold-blooded calculations, while the best instincts of humanity would heartily applaud the disregard.

Putting sentiment and natural affection entirely aside, it is

obvious that the health of the individual is relative to his environment, and that, as the race is made up of individuals, the improvement of surroundings which help one helps all. Bright sunshine, pure air, a roomy well-ventilated house, a clean city, good milk and food, are not helpful merely to the sick and weakly. Let us take the births of a year in Great Britain at a million, and let us assume that in inferior health conditions one-tenth of these will, during the first ten years of life, fall below the line of viability—will cease to live. That, it may be urged, is a good thing, as they are better dead, being unfit to remain. Let us suppose, however, that the environment is improved, so that this tenth remains above the line of viability—survives into manhood and womanhood. That, it may be said, is bad, as it tends to propagate an unhealthy race. But let it be observed that when this tenth is raised from non-viability into viability, all the superimposed nine-tenths of the million are raised through a similar space in our imaginary scale. The second lowest tenth, which formerly rested on the level of viability, is now one place higher, and so throughout the column. The gain is nine times the loss. And if, in obstinate determination to get rid of that unhealthy tenth, we return to the old and inferior conditions of life, it is true that the lowest tenth goes below the line, but the second lowest takes its place. If pursuing this course we determine to get rid of the second tenth by making still harder its struggle for life, no doubt we succeed, but then the third tenth takes the place of the second, and so on until, endeavouring to improve the species, we exterminate the race. Fortunately in this matter science and natural affection, social economy and philanthropy, go hand in hand. The quality of mercy is not strained, and the same conditions which strengthen the weakest make also for the survival of the fittest.

CLINICAL LECTURE

ON THE OPEN-AIR TREATMENT OF PULMONARY TUBERCULOSIS

Delivered to the Class of Clinical Medicine in the University of Edinburgh, November 1899

By R. W. PHILIP, M.A., M.D., F.R.C.P.Ed.,

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I PROPOSE asking your attention to-day to a comparatively recent departure in therapeutics, viz. the open-air treatment of pulmonary consumption. The subject suggests itself for a variety of reasons, of which I shall mention one or two.

Firstly, the frequency of the occurrence of, and the high mortality from, pulmonary tuberculosis are well known to you. Let me remind you that there seems sufficient pathological evidence for the statement, that approximately one-third of all persons living become affected by tuberculosis in some form. Of all persons dying during the period of greatest working activity—say from fifteen to forty years of age—about one-third die of pulmonary consumption. In England and Wales, some 40,000 persons, and in Scotland some 7000 die annually from the disease. Of every 100,000 persons living, approximately 200 die of consumption. In many districts consumption kills more persons than do all the zymotic diseases together, as the following tables illustrate.

I. TABLE showing Mortality in Edinburgh from Pulmonary Consumption and Fevers during ten years, from 1887 to 1896.

	Deaths from Consumption.	Deaths from Fevers.		Deaths from Consumption.	Deaths from Fevers.
1887	491	599	1892	480	641
1888	458	306	1893	465	459
1889	459	419	1894	469	395
1890	532	695	1895	555	511
1891	513	384	1896	455	277

II. TABLE showing Mortality throughout German Empire from Pulmonary Consumption and Fevers in 1894.

Deaths from Pulmonary Consumption,		115·034
Deaths from Fevers—		
Diphtheria and Croup,	63·701	
Whooping Cough,	21·521	
Measles, . .	16·173	
Scarlatina, . .	8·937	
Typhus, . .	6·373	— 116·705

N.B.—If to the deaths from consumption be added the deaths from other forms of tuberculosis, it brings the figure up to 123·904.

In considering these tables, it is of importance to bear in mind further, that while the fevers run a short decisive course, and occur most frequently in children, the duration of consumption is prolonged—it may be throughout years—and its victims are chiefly grown persons at the most active period of life.

These statistics suffice to emphasise the significance of consumption in relation to general practice, and indicate the important part the disease will play in your daily work as doctors.

In the *second* place, I have chosen the subject because of the comparative freshness of the method of treatment, at any rate so far as this country is concerned. As we shall see presently, however, the conception of the method, even in this country, is far from novel, although practically it cannot be said to have been followed out with any degree of thoroughness.

Thirdly, in the Royal Infirmary, you have no opportunity of studying the system. It is obviously impossible that the method should be practised satisfactorily. The wards and general arrangements are planned in the interest of patients suffering from a variety of diseases, often of more acute character. So soon as the acuter phase of the complaint is past, and the patient is convalescent, we are anxious to get him as speedily as possible out of the hospital. Moreover, the situation of the Infirmary in the heart of the city renders it less suitable on other grounds for the practice of out-door methods.

Fourthly. In Edinburgh we are fortunately placed in relation to this treatment. During the past five years it has been practised continuously, and has thus been fully tested in the Victoria Hospital for Consumption, where the "*Cure à l'air libre*" has been the regime from the commencement. I take this opportunity of inviting as many of you as feel an interest in the matter to visit the hospital, and see for yourselves the details of the treatment.

Historical

The open-air treatment of pulmonary consumption is not quite so recent a method as some statements published regarding it might lead you to suppose. It was, in fact, advocated many years ago in this country, and then, doubtless, practised quietly by some of its zealous advocates. Notably Dr George Bodington in 1840 published an essay on "The treatment and care of Pulmonary Consumption," in which open-air methods were strongly urged. A few years later Dr Henry MacCormac published a remarkable work on "Consumption, as engendered by re-breathed air." Curiously enough, one chapter in that volume was first read as a communication to the Edinburgh Medico-Chirurgical Society in 1856. In that essay there occurs the pregnant sentences:—

"If the inhabitants of Great Britain and Ireland would but consent, day and night, to live in the pure and unprebreathed atmosphere, it would put a total close to the ravages of consumption and scrofula, white swelling, tabes mesenterica, water on the brain; in fine, to the whole abhorred family of tuberculous disease. The very purest air, by day, will not suffice without pure air by night, also. . . . If the nights be passed in some stagnant, airless chamber, it will too surely consummate the work of destruction. We must do away with our curtains, pull down our glass screens. Windows are meant for the admission of unprebreathed atmosphere, and not merely to keep it out. . . . We spend so much of our short life in bedrooms, that with a pure bedroom atmosphere, pure and fresh, in fine, as is the outer atmosphere itself, for here nothing else will suffice, there would, as I affirm and maintain, and, with all my convictions believe, with reasonable care and atten-

tion otherwise, be no tubercle, and consequently no scrofula, no consumption whatever."

Elsewhere in Europe the physiological conception of treatment took practical shape. Especially is this true of Germany, where the open-air, or sanatorium treatment in stricter sense, came into vogue through the labours and teaching of the late Dr Brehmer. Brehmer made a commencement at Görbersdorf, in Silesia, and the sanatorium grew quickly because of the successful results which he obtained. His disciples spread throughout Germany, and other sanatoria were founded. During the last few years the growth of these sanatoria has been comparatively rapid in different parts of the Continent. In the United Kingdom, however, the practical elaboration of the method has been slower. Within the past year or two more attention has been directed towards the system, and already a considerable number of sanatoria have been established in this country. It has been my privilege to have been associated with the initiation and working of the system at the Victoria Hospital since its commencement five years ago, and what I have to say on the subject is based on close personal observation throughout that period.

Principle of the Method

The open-air method—it matters not by what name you call it—is in large part a reversal of the ordinary methods of treating consumption. These methods, you know, had for their keynote, protection, more especially from the changeable atmospheric influences which characterise the climate of these Islands. In the newer method, this protective policy has been reversed.

While the effects of climate, both favourable and prejudicial, are not ignored, it is now recognised that certain factors, formerly dwelt on as essentials to treatment, must no longer be regarded in that light. Thus, mere *elevation*, as such, has less to do with determining success in treatment than used to be supposed. Consumption is treated successfully at all levels—from 6000 feet above to some distance below sea level. *Temperature* likewise must be regarded as of slight importance. This is shown by the varying mean of temperature recorded at various stations where the disease is under treatment. Similarly neither *dryness* nor *moisture* of the atmosphere, though

PLATE I.—CONTINUOUS OPEN-AIR TREATMENT FOR PATIENT CONFINED TO BED.
(*Victoria Hospital for Consumption, Craigleith, Edinburgh.*)

PLATE II.—CONTINUOUS OPEN WINDOW.

significant in relation to particular cases, can be considered of essential moment. The *character of the soil* is a matter deserving much consideration in relation to the selection of a place of residence for the consumptive patient. The dryer the soil—other things being equal—the better for the patient. But in view of the geographical distribution of the disease, this must not be placed in the list of essentials. The disease prevails in all places where it has obtained a footing, and can be treated with approximately like success in all climates, according as the major indications for treatment are fulfilled or not. This view is of vast importance economically, and can hardly be emphasised too much. For the tuberculous patient, in whatever station of life, it is immensely comforting to realise that he may be satisfactorily treated in, or at least not very far from, his own home.

In what does the treatment consist? While there are many accessories of importance, the open-air treatment consists primarily in the free exposure of the patient to "open air." He must be bathed in fresh air day and night quite irrespective of the nature of the weather. Nothing must be accepted as an excuse for a reversal of the policy—neither rain, nor snow, nor wind, nor even mist. Throughout a prolonged experience, I have not on any one occasion seen the slightest accident occur in relation to the freest exposure of the patient day and night to this influence. Of the benefits which occur I shall speak presently.

Can there be too much exposure of the patient? Short of actual chill, which may be avoided by suitable clothing and shelter, it is hardly conceivable that there can be too much—either by day or night.

The Method in Practice

Let me ask you to consider with me more in detail the method of putting the open-air treatment into practice. It will be convenient to discuss it thus (*a*) By day and (*b*) By night.

(*a*) *By day.*—During the day, the patient should be outside entirely, resting either on a couch (we sometimes carry the bed out entirely, Plate I.) or on a reclining chair, or other seat, according to the stage of illness. At the commencement of treatment, and in more pronounced cases for a longer period, the major part of the day should be spent resting. Some

patients must rest throughout the entire day. When for any reason, such as the presence of a complicating diarrhœa, or the like, it is inconvenient to have the patient outside, the bed should be advanced to the widely opened window (Plate II.). Although I have had many patients treated for long periods at such an open window, I have never heard a single complaint either of cold or draught. On the contrary it is wonderful how quickly the patients come to like, and even to revel in, the treatment.

When the patient is resting outside, arrangements must be made for the exclusion of draughts, and more boisterous winds, and—if you will—rain. This may be managed by a variety of simple devices, most of which can be provided in relation to any house, with a small garden. Thus, for example, simple wooden screens may be attached to the wall of the house, and made to swing on a hinge, so as to be fixable at any angle. Plate III. illustrates this most available method. Such draught or wind screens we have found of extreme value. Or, more formal shelters may be built of wood and glass. These should be roofed with glass. The ends may also be conveniently made of the same material. The shelters should not be too deep, and the roof should slope from the back upwards, in order that there may be as little opportunity as possible for the collection of foul air. Plate IV. shows such a shelter. Similarly, rotating chairs, such as are provided for yachts, are available for our purpose. These are made to swing round so as to guard the occupant from rough winds. They can be readily altered in position by the patient himself. Plate V. illustrates these. I need hardly say that larger shelters are easily made on this rotating principle.

How long may the patient remain outside? My answer is, the longer the better. I have seen no mischief accrue from most prolonged exposure in all weathers. As we all know, Edinburgh is not credited with the most perfect of climates. The climate is unduly maligned. Without giving ourselves away, we may safely admit that our spring weather is not always delightful. The following table records the length of time that the patients at the Victoria Hospital enjoyed out-of-door treatment during the month of March of the present year.

PLATE III.—SCREEN SHELTERS, ADJUSTABLE TO ANY ANGLE.
(*Victoria Hospital for Consumption, Craigleith, Edinburgh.*)

PLATE IV.—COVERED SHELTER.
(*Victoria Hospital for Consumption, Craigleith, Edinburgh.*)

TABLE SHOWING DAILY PERIODS IN HOURS DURING WHICH OPEN-AIR TREATMENT WAS ENJOYED OUTSIDE BY EACH PATIENT IN THE VICTORIA HOSPITAL FOR CONSUMPTION DURING MARCH 1899.

Date.	Total Hours of Sunshine.	I.	II.	III.	IV.	V.	VI.	VII.	VIII.	IX.	X.	XI	XII.	XIII.	XIV.	XV.	XVI.	XVII	XVIII	XIX.	XX.
March 1	2·55	6	7½	7½	2½	5½	7	5½	6½	7½	6	7	5½	7	...	...	...	In Bed, alongside widely open window all day.	In Bed, alongside widely open window all day.	...	...
,, 2	...	6	7½	7½	3½	5½	6½	5	5½	7½	5½	7½	6	7½	...	...	...			...	...
,, 3	3·95	5½	7½	7½	4	6	7	5	6	7½	6	7½	4½	7½	...	...	...			...	...
,, 4	3·95	6	7½	7½	4	5½	6	5	6½	7½	6	7½	5½	7½	...	...	...			...	...
,, 5	·15	5	7	7	2	5	6½	5	5½	7	6	7	5	7	...	...	...			...	...
,, 6	3·10	5½	7½	7½	5½	5½	6½	5	6	7½	5	7½	5	7½	...	...	...			A	...
,, 7	7·35	5½	7½	7½	4	5½	6½	5	6	7½	6	7½	4½	7½	...	...	...			5½	...
,, 8	·38	6	7½	7½	2	5½	6½	5	6½	7½	6	7½	5	7½	...	...	...			5	...
,, 9	...	6	7½	7½	4	6	7	5½	6½	7½	6	7½	4½	7½	...	...	...			5	...
,, 10	·15	5½	7½	7½	2	6	6½	6	6½	7½	6	7½	5½	7½	...	...	...			3½	...
,, 11	·10	6	7½	7½	2	7	6½	6	7	7½	6½	7½	5	7½	...	...	...			5	...
,, 12	7·82	6	7½	7½	3	5½	7½	7	7	7½	6½	7½	6½	7½	...	...	...			5½	...
,, 13	·65	6	8	8	4	5½	7	5½	6½	7½	6	8	5½	8	...	...	...			6	...
,, 14	1·36	6	8	8	4	5½	7½	6	7	7½	6	8	6½	8	...	...	...			6½	...
,, 15	...	6½	8	8	4	8	7	5½	7	8	8	8	6	8	...	...	...			6½	...
,, 16	5·72	7½	9½	9½	6½	5½	9	8	9	9	9	9	9	9	...	...	...			9	...
,, 17	8·48	8	9½	9½	7	7½	9	7½	8	9	9	9	6	9	...	...	...			9	...
,, 18	7·77	D	8½	8½	6	6	7½	6	7½	8½	8½	8½	6½	8½	A	...	...			7	...
,, 19	2·05	...	8	8	3	5	6½	5	6½	8	8	8	6	8	8	...	...			6	...
,, 20	2·55	...	8½	8½	1	5½	7½	5½	7	8½	8½	8½	7	8½	8½	...	...			7	...
,, 21	6·02	...	9	D	3	6	7	6	7½	9	9	8½	7	8½	9½	A	...			7½	...
,, 22	8·02	...	9½	...	2½	6	D	6	8	9½	9½	9½	8	9	9½	9½	A			8	...
,, 23	7·67	...	D	...	4	5	...	7	8	9½	9½	9½	8	9½	9½	9½	6			8	...
,, 24	5·32	...	...	...	6	4	...	7	8	9½	9½	9½	8	9½	9½	9½	8			8	...
,, 25	...	...	...	...	3	...	...	6	6	D	D	7½	6	7½	7½	8	7			6	...
,, 26	...	...	...	...	...	2	...	5	5½	...	...	5½	5½	5½	4½	5	6			6	...
,, 27	3·41	...	...	...	3	3½	...	7½	D	...	...	8½	7½	8½	8½	8½	7½			7½	...
,, 28	·83	...	...	...	5½	...	...	6	...	...	...	7½	6	7½	7½	8½	7			6	...
,, 29	6·85	...	...	...	4	...	...	7	...	...	...	9½	6	9½	9½	9½	8			8	...
,, 30	·77	...	...	...	7½	3	...	7½	...	...	...	9	7½	9	9	9	7			7½	...
,, 31	...	...	...	...	1½	...	...	5½	...	...	...	8½	2	8½	8½	8½	6			5½	...

Roman numerals indicate patients. A means admission, D discharge.

I have selected the month of March, because it indicates the possibilities during the least clement part of the year. You will agree with me, I think, that the patients were out of doors far more freely than the vast majority of our healthy citizens, and this only with benefit to themselves.

In all cases where the patient is outside it is necessary to watch carefully that the peripheral circulation is well maintained. The patient himself, or the nurse, or the doctor should see that the finger tips and toes are really warm.

(*b*) *At night.*—During the night the windows should remain freely open. The existing popular dread regarding night air is quite mistaken. There is abundant reason to maintain that night air is in all respects good, and in certain respects better than the air by day. Even in stormy weather free entrance of air may be effected between the window sashes without draught by a very simple plan—namely, to raise the lower sash some four to six inches, and to introduce between its lower edge and the window sill a piece of board accurately fitted to the width of the window. The lower sash rests on this, and thereby a continuous and gentle upward current of air passes between the sashes into the room. In calm weather this arrangement will be supplemented by the free lowering of the upper sash. Similar arrangements can be made with a casement window.

How is the patient to be occupied while out of doors? This must vary in different cases. At the start, it is wise to insist on the greater proportion of patients resting entirely. Where the temperature swings much, and particularly where the circulation is prejudiced, the more the patient rests the better. With improved condition, a certain amount of activity will be permitted. Such activity is to be increased gradually. Thus the patient may be enjoined to take ten to fifteen minutes' gentle walk each hour, resting for the remainder. Stronger persons will be permitted to walk gently for an hour or two in the forenoon, and for a similar period later in the day. In all cases, rest for half to three-quarters of an hour should be insisted on before meals, and, say, an hour after meals. Walking exercise, especially at first, must be studiously quiet—at the rate of not more than two to three miles an hour. To commence with, it will be wisely carried out on the flat, and, with growing strength, on gradually increasing inclines.

PLATE V.—ROTATORY CHAIR SHELTERS.
(*Victoria Hospital for Consumption, Craigleith, Edinburgh.*)

PLATE VI.—VICTORIA HOSPITAL CHEST EXPANDER.

(*Victoria Hospital for Consumption, Craigleith, Edinburgh.*)

So far, we have dealt with the *essential* of the new system —namely, the free exposure to open air. Before leaving this, let me remind you that, while this may be effected best in a sanatorium, specially devoted to the purpose, the physiological principle which underlies the method is capable of application almost anywhere. A garden of comparatively small dimensions may be readily adapted to the purpose. Even in a dwelling-house without such garden, a part at least of the benefit may be attained, if the physiological indications have been fully apprehended.

Ajuvant factors in the treatment

I have spoken of open air as *the essential* of the method, for the very good reason that without the free use of fresh air, the other factors to be mentioned are comparatively useless. These factors include :—

(*a*) *Diet.*—This is an important point. Special attention has been drawn to it under the name of "forced feeding." This term seems to me a misnomer, because, if the larger physiological condition be fulfilled, there is commonly little need to force the patient to eat. In proportion as the rules regarding fresh air, day and night, are followed, the patient becomes more or less rapidly hungry, and even voracious. Contrariwise, when the essential is neglected, forced feeding does little good, and may do a deal of harm. I believe that one of the reasons why breakfast is the meal usually least enjoyed by the ordinary patient is to be found in the fact that he has, for from seven to ten hours, been confined in a more or less completely closed room. My experience has been that, under the system we are discussing breakfast is as greedily enjoyed as the other meals.

It is impossible for me in a single lecture to go into details of dietary. Let me say, however, that where the patients are not unusually ill, it is best to give three or four good simple meals *per diem* at sufficiently distributed intervals. It is unnecessary, and even prejudicial, to give much between meals. In the case of more delicate subjects, the meals, which are less heartily taken, must necessarily come more frequently.

What about stimulants? In many cases, alcohol is unnecessary. Where the digestion is sluggish, and the patient

apt to suffer from flatulence and discomfort, a tablespoonful of whiskey or similar stimulant, taken along with meals, is often helpful. Similarly a glass or glass-and-a-half of light wine—moselle or hock—may sometimes be taken with advantage. In delicate patients, with pyrexia and disturbed circulation, alcoholic stimulation is commonly needful.

(*b*) *Clothing.*—The clothing of the patient, both by day and night, should be as light as is compatible with sufficient warmth. The preposterous overclothing of the body, so frequently seen, must be stopped. At no season should more than a single shirt and an under woollen garment be commonly permitted, in addition to the upper suit or dress. Chest protectors and swaddling bands of cotton wadding, so frequently seen, are to be discredited entirely. It is quite ridiculous to count the number of layers one has to peel off many consumptive patients before reaching the chest wall. Such absurd clothing removes the last chance for the skin as a heat regulating mechanism. By night the same general principle is applicable in respect of clothing.

(*c*) *Exercise.*—I have already referred to the necessity of rest, at first, in all and, for a longer period, in the more pronounced cases, and the necessity for graduating carefully even walking activity. With advancing improvement, however, a certain amount of more definite exercise is to be recommended. Pretty early in the treatment, we are in the habit of making use of a special form of chest expander (Plate VI.). This is applied during ordinary walking exercise for an hour or two, or longer, daily. By means of this apparatus, respiration is rendered easier and fuller. In young subjects more particularly, defects of chest architecture are thereby effectively remedied. In other cases, wisely selected movements of the arms will be sanctioned. These should be such as tend to increase the respiratory capacity. To this end, they must be practised slowly. If dumb-bells are added, they should be quite light. Still later, when improvement is more pronounced, mild golfing exercise and the like may prove beneficial. The patient must be guarded scrupulously against over-fatigue and against chills after over-heating.

(*d*) *Bathing.*—The skin of the tuberculous patient should be freely cleansed once or twice daily. In cases of pronounced weakness, we may content ourselves with sponging and dry

rubbing of the surface. In the larger proportion of cases, however, it is much more satisfactory to allow a rapid dip into a full bath, the temperature of the water being regulated according to the season, so as to avoid undue shock. I have frequently observed the first indication of real improvement when such bathing was commenced. To be serviceable, care must be taken that the bath is short, and that the patient is comfortably dried—best of all by an assistant—thereafter.

(*e*) *Drugs.*—For our present purpose it seems wiser to refrain from formal discussion of drug treatment. In proportion as the other indications are fulfilled the drug treatment will take a second place.

To what class of patient is the method applicable?

The answer to the question is that there is no limit to the application of the method, provided there be a wise discrimination in respect of details. The results are, of course, very different in degree. Satisfactory—that is, lasting—results can only be expected in cases of pulmonary tuberculosis which are taken in hand sufficiently early. This emphasises strongly the desirability of early diagnosis. According as the patient is taken sooner or later in hand, must be not only the effect, but also the length of the necessary treatment. I have had an opportunity of testing the significance of the method in all stages, and wish before entering on details to emphasise the fact that while the degree of benefit has necessarily varied, in no single case has a positively bad result followed.

Results of Treatment

In order to appreciate more certainly the character and degree of benefit to be expected, let me remind you of a few of the more salient features of pulmonary tuberculosis. These include particularly the pallid, wasted, perhaps hectic, aspect of the patient, emaciation, which is probably progressive, cough, expectoration of different sorts, dyspnœa, loss of appetite and dyspepsia, pyrexia, circulatory prejudice, and night sweating. This list might be added to, but I take the symptoms named for the purpose of illustration.

Let us see how these are modified under the open-air treatment.

(*a*) *The colour and general appearance* of the patient are quickly changed. The pallid, delicate, and perhaps hectic aspect is replaced in the course of a few weeks by a ruddy, healthy look, as you may judge for yourselves from the patients whom I show you. One of the most remarkable experiences in relation to this treatment is that patients coming to us from country districts, such as Berwickshire, Lanarkshire, Fife, Perthshire, and even the Highlands, regain colour in Edinburgh just as they would regain colour elsewhere, if the proper method were followed. There gradually develops a robustness of look similar to that observable on ship-board. I have often remarked, on going round the hospital, how approximately correctly one may fix the length of residence by the general appearance of the patient.

(*b*) *Weight.*—The loss of flesh which has probably been progressive is quickly replaced by a rapidly progressive gain in weight. Such gain is at times phenomenal. We have frequently recorded an increase at the rate of 3 to 4 lbs. per week, and a gain of from 14 to 28 lbs. is quite common during the first eight or ten weeks. At the present time there is only one patient actually in residence with us who is not steadily putting on weight.

(*c*) *Cough and Expectoration.*—It is astonishing how quickly cough disappears. Visitors to the hospital have again and again remarked to me how infrequently the patients were heard to cough. This in itself is an enormous benefit, both to the patient and to those around him. Thereby the spread of the process within the lung is limited, and the tendency to emphysema is reduced. The expectoration also tends to lessen, and in many cases is reduced so much as to be hardly calculable. Its final disappearance necessarily takes time.

(*d*) *Dyspnœa.*—Breathlessness similarly yields. Patients who on their entrance have difficulty in creeping upstairs, or even crawling into bed, recover their breath in a marvellously short time. In many cases the total disappearance of this symptom cannot be looked for, having regard to the process of consolidation and shrinkage which must remain in large part.

(*e*) *Loss of Appetite and Dyspepsia.*—I have already referred to the extraordinary change which occurs in this matter. The loathing of food which is so frequent quickly disappears.

At present, for example, there is not a single patient in the hospital who has not a healthy and even a greedy appetite. Dyspeptic manifestations commonly yield also. The more fully

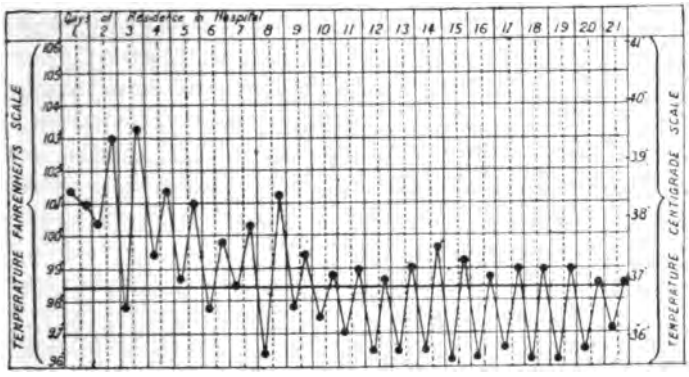

Days of Residence in Hospital
1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17 18 19 20 21
TEMPERATURE FAHRENHEITS SCALE
TEMPERATURE CENTIGRADE SCALE

CHART I.

the tuberculous patient is exposed to the fresh air, the less does he require of special medicines directed to his stomach. With increasing activity, the constipated habit of bowel which is common in the disease tends to alter.

(*f*) *Night Sweating.*—From time immemorial night sweat-

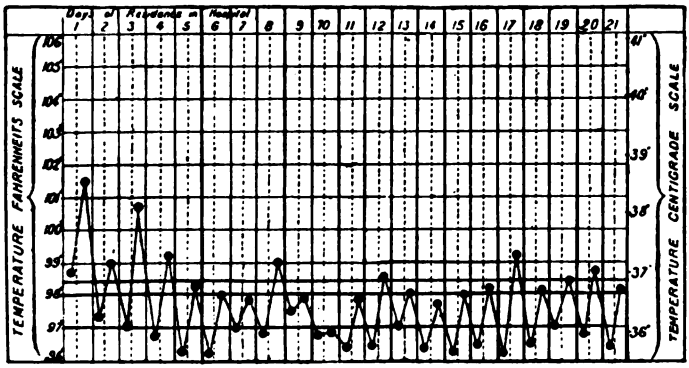

Days of Residence in Hospital
1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17 18 19 20 21
TEMPERATURE FAHRENHEITS SCALE
TEMPERATURE CENTIGRADE SCALE

CHART II.

ing has been cited as one of the characteristic symptoms of the disease. Yet strange to say, at the present time—and indeed, as a general rule—we have not a single case of night sweating

in the hospital. Patients frequently come to us whose leading symptom has been night sweating. For this they have required to be changed three, four, or even six times a night, with corresponding disturbance of rest. Within two or three weeks, the symptom almost certainly disappears, and this, you understand, without the use of drugs directed towards this end.

(*g*) *Circulatory Disturbance.*—One of the most satisfactory effects of the treatment is the speedy improvement which takes place in connection with the circulation. The rapid, excitable, soft pulse is replaced by a slow, steady and strong beat. I have frequently observed within two or three weeks a pulse of 100 or more falling to 80, or even lower.

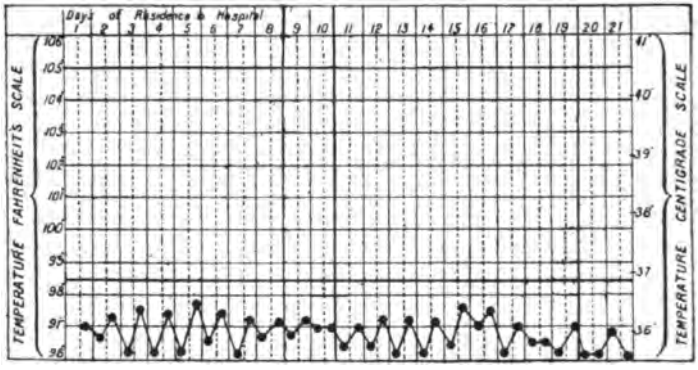

Days of Residence in Hospital
1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17 18 19 20 21
TEMPERATURE FAHRENHEIT'S SCALE
106 105 104 103 102 101 100 99 98 97 96
TEMPERATURE CENTIGRADE SCALE
41° 40° 39° 38° 37° 36°

CHART III.

(*h*) *Temperature.*—Pyrexic manifestations yield as a rule, and in some cases, marvellously. In a very short time, temperature charts which have shown a violently swinging curve, are modified until soon an approximately normal curve is registered. I show you several charts illustrative of this point. Thus charts I. and II. show such an alteration from a swinging pyrexial elevation to the approximately normal, within ten days after admission, although for weeks prior to admission in each case the patient's temperature had been reported to be swinging between 102° and 97°. It is indeed not common in my experience to note pyrexial disturbance in patients who have been in residence for any length of time, unless the condition is complicated by the involvement of other organs, or

the like. The most frequent type of temperature chart, which we have had repeated time after time, shows a curve slightly below the normal line (Chart III.).

In this connection, let me warn you regarding the necessity for registering the temperature frequently throughout twenty-four hours before concluding that the temperature curve has reached the normal. If you limit yourself to temperature readings morning and evening only, or even three times a day, you are apt to fall into error.

From what I have been able to place before you to-day, I think you will realise that not only is the open-air treatment of consumption worthy of most thorough trial, but that the results which have followed its adoption throw a flood of light on our etiological conception of the disease, and enlarge enormously the horizon of preventive medicine so far as pulmonary tuberculosis is concerned.

THE TREATMENT OF TEAR DUCT OBSTRUCTION[1]

By ANGUS M'GILLIVRAY, C.M, M.D., F.R.S.E.,

Lecturer on Ophthalmology, St Andrew's University; Ophthalmic Surgeon, Dundee Royal Infirmary; Surgeon, Dundee Eye Institution; Ophthalmic Surgeon, Dundee Institution for the Blind

THIS being one of our clinical evenings, I shall not attempt to lay before you the various methods recommended for the treatment of this troublesome affection, but shall content myself with describing the treatment I have found most suitable.

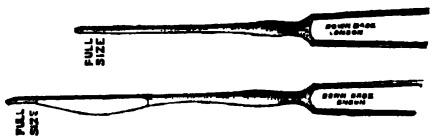
The treatment usually adopted at the present time consists in opening either canaliculus, and passing probes of various sizes with or without syringing the duct with astringent or antiseptic solutions. To be successful the probing and syringing must be repeated some dozen times if anything like permanent relief is to be obtained. This means about a dozen more or less painful operations, and, especially in the case of patients from a distance, considerable inconvenience.

For the past three years the treatment I have invariably adopted consists in opening the lower canaliculus, passing a

[1] Read before the Forfarshire Medical Association.

number 6 Couper's probe, and keeping the duct open by means of a specially constructed solid metal style, which is not removed for two or three months.

Let me illustrate, by taking as a typical example the treatment of a case of tear duct obstruction with mucocele. A 5 per cent. solution of cocain is dropped into the lower conjunctival cul-de-sac every three or four minutes for twenty minutes to produce local anæsthesia. The anæsthesia may be increased by injecting with an Anel's syringe, or a blunt hypodermic needle, a few drops of a weaker solution of cocain into the lachrymal sac five minutes or so before operating. The canaliculus is dilated by means of a fine probe, and the knife is then passed along till the blunt point comes in contact with the nasal bone; the knife is now turned with a circular movement from the horizontal to the vertical position, and passed down the tear duct, so as to divide the stricture, which is usually found at the upper end. The knife is withdrawn and a number 6 Couper's bulbous probe inserted. This in turn is withdrawn, and the style passed into the duct, the whole operation occupying only a few seconds. The style I employ is made from electric fuse wire of 2 mm. calibre, and is composed principally of tin with a small proportion of lead. It is made by cutting off a piece of this wire about 3·50 cm. long, rounding the ends with a fine file (say that of a penknife), and bending half a centimetre at one end to a right angle. The advantages of this material are its toughness, cheapness, and adaptibility to the direction of the duct. As regards the knife, Weber's is the one generally employed, but I prefer the knife I now show you, which was made for me by Down Brothers. It consists of a probe at one end, and a knife at the other. The knife, as you will see, is essentially a flattened conical probe, having one of the edges sharpened about ·50 cm. from the point. The conical shape of the cutting edge allows the canaliculus to be divided accurately while the knife is being passed along it—a matter of importance when it is desired to make the inner ridge of the divided canaliculus lower than the outer. Further, the straight conical-shaped point facilitates the



entrance of the knife into the stenosed portion of the duct, a distinct improvement on the curved point of the Weber's knife.

When the style is inserted, the bent portion at the upper end should lie in the divided canaliculus, without causing the slightest pressure at any point, and should hold the cut edges of the canaliculus slightly apart. If care be taken to have the inner ridge of the divided canaliculus slightly lower than the outer, the portion of the style lying in the canaliculus cannot be seen unless the lower lid be everted—an undoubted cosmetic advantage.

There is no necessity for syringing the duct either before or after the operation, and the subsequent treatment consists simply in bathing the eye with a slightly astringent lotion, such as alum 18 grs. and boric acid 60 grs. to eight ounces of water, and once or twice a day emptying the lachrymal sac of its mucous or muco-purulent contents by pressing over the sac with the point of a finger.

If the style be properly constructed and lie comfortably *in situ*, the patient need not return for a couple of months or more. At the end of a week or two the muco-purulent nature of the contents of the sac, even in a case of recent lachrymal abscess, changes into mucus and shortly afterwards into tears.

The style is generally removed in three months, being found practically clean and unchanged, and the astringent lotion continued for a month or so longer. I advise the patient to empty the sac regularly in the morning, and lave with cold water.

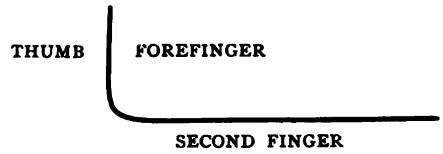
This treatment I have applied in about two hundred cases with such gratifying results that I am not likely to return to the method of probing.

NOTE ON A NEW SHAPE OF NEEDLE

By CHARLES A. STURROCK, M.A., M.B., F.R.C.S.Ed., Dunfermline

THE objection to a straight needle is that one is sometimes unable, without the aid of a needle-holder, to push it through dense tissues; and the point of a curved needle is sometimes erratic, emerging where it was not intended to emerge; I have, therefore, had made for me a needle which overcomes these disadvantages.

A little practice makes one deft in its use; it is suitable for sewing wounds where the skin is thick, but can also be used for wounds in other regions; and, in order that the



passage of the needle through the skin may be in a line at right angles to the surface of the body, it should be pulled through one edge of the wound before being inserted into the other, each edge being held up with dissecting forceps.

The needle can be obtained in various sizes, and the point may be round or bayonet-shaped.

I have used such a needle for over five years, and Mr Gardiner, the instrument maker, has made it for me.

Special Article

"HAUNTED HOUSES"

IT may be within the recollection of readers of the *Scottish Medical and Surgical Journal*, that a number of letters appeared in *The Times* during June 1897 having reference to the alleged haunting of a country house in Scotland. These letters were followed by an article in *The Nineteenth Century* of August 1897 by "Miss X." (Miss Goodrich-Freer), a lady well known in the field of experimental psychology as an authority on Scrying or crystal-gazing, and for her researches regarding Second Sight in the Highlands.

This lady, early in this year, in the publication of her "Essays in Psychical Research," threw off her anonymity, and now appears as the co-editor with the Marquis of Bute of a book entitled "The Alleged Haunting of B. House,"[1] in which they give a very full and carefully digested account of the subject, not, as they modestly say, "to establish theories but to record facts."

The reader who takes up the book in the expectation of finding a thrilling romance after the style of that inimitable ghost story of Bulwer Lytton's, "The Haunted and the Haunters," is doomed to disappointment; for here we find a

[1] London: George Redway, 1899. 250 pp., price 2s.

cold and critical survey of facts which precludes the possibility of its being merely a good story. The book is, nevertheless, extremely good reading, both from the matter it contains and its literary style.

Its history is as follows:—Lord Bute is a Vice-President of the Society for Psychical Research, and takes a warm interest in its proceedings. In 1892 he heard from a priest, Father H., of certain curious phenomena which he had experienced whilst staying at B. House shortly before. Further inquiry showed that the house had long had a reputation in the neighbourhood of being haunted by the ghost or spirit of a former proprietor, Major S., and seemed to offer a justification for scientific investigation of supernormal phenomena should opportunity present itself. This opportunity was afforded in 1896 by the death of the owner, and the house being let furnished by his successor. It was hired on behalf of Lord Bute by Colonel Taylor, also a prominent member of the Society for Psychical Research, in January 1897, and Miss Goodrich-Freer, an Honorary Associate of that Society, was asked to act as chatelaine.

This lady and her friend Miss Moore took possession on February 3rd, and for the next four months kept the house filled with relays of guests, a few of whom were selected because they were practised observers of psychical phenomena or were interested in occult matters, but the greater number were asked merely for a country house visit in order that the inquiry might be made without *parti pris*.

The previous occupiers had experienced so much annoyance and alarm from ghostly phenomena that after a few weeks' residence they had forfeited their year's tenancy, and the occasion seemed most promising. Several eminent men of science, including Professor Oliver Lodge, F.R.S., and Sir James Crichton Browne were among the visitors, and it must be confessed that never had a ghost a better opportunity of proving its actuality. It is a matter of regret that it did not avail itself of it after a more sensational fashion. But though the phenomena observed hardly came up to expectation, they were interesting, and as typical of a large class of similar cases, are worthy of consideration here.

Though there were local rumours of earlier haunting, the phenomena may be taken to date from the death of Major S., which occurred in 1876. He was greatly attached to the place and lived a somewhat eccentric life there, devoted chiefly to the care of his dogs. He was unmarried, but there was some scandal about a young woman who lived in the house as housekeeper and who died there in 1873.

As an instance of his eccentricity it is stated that Major S. believed in metempsychosis, and that he declared his intention of entering after death the body of a favourite black spaniel.

On his decease his heir had all the dogs—numbering fourteen—shot. This heir, his nephew, a Roman Catholic, was killed in a cab accident in London in 1895, and the property passed to his son, Captain S., the present owner.

The house is a comfortable modern mansion situated in large grounds, and is of a very unromantic appearance. A plan of it is given in the book, and the rooms are indicated which appeared specially subject to ghostly manifestations. Most of the phenomena occurred in the house, but the apparition of a young woman dressed as a nun accompanied by another figure, that of an older woman dressed in grey, were frequently seen by Miss Freer and five or three other visitors at a certain spot in the grounds. Miss Freer was throughout the chief percipient, partly perhaps because her long stay gave her greater opportunity than others, and several of the visitors neither saw nor heard anything abnormal. Lord Bute, during the two days he spent there, experienced nothing but a "feeling of discomfort as from the presence of a hostile force," and a difficulty of articulation, especially in certain rooms, when he repeated forms of prayer used in exorcism of evil spirits by the Roman Church.

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Finally on May 6th the services of a bishop and two priests were called in, and they stayed at the house and said mass there. During the Service three of those present saw the figure of the nun at the window, "looking pensive but much relieved." After the Service the bishop and priests went over the house and in each room sprinkled holy water and gave a blessing. There was a loud bang on one of the tables, and that was the last of the phenomena. Miss Freer and some of her guests stayed a week longer, but nothing occurred.

The editors summarise the phenomena observed as follows:—

1. *Visual.*—(*a*) The apparition of the nun already referred to. This was seen frequently by Miss Freer and Miss Langton, and by "Mr Q." an English rector. (*b*) An elderly woman dressed in grey, generally seen in company with the preceding, and seen several times by Miss Freer and Miss Langton. The two figures seemed to be generally in conversation, and the noise of their voices was heard by four other visitors. (*c*) The apparition of a brown wooden crucifix seen by Miss Freer and "Mr Q." during this tenancy, and by Father H. previously. (*d*) The figure of a woman seen by Miss Freer alone on one occasion in the drawing-room. (*e*) The same phantasm was apparently on one occasion seen and described by a housemaid as appearing by

her bedside at dawn on April 27th, and was described by earlier witnesses also. (*f*) The apparition of a dog was seen by Miss Freer and Miss Langton on one occasion, and was apparently that of a black spaniel. Miss Freer's black Pomeranian also seemed to be affected by this vision and was evidently much terrified by it.[1] (*g*) The phantasm of Father P. H. appeared to Miss Langton one night. She had never seen him, but the resemblance was so complete that Miss Freer and Miss Moore, who were also strangers to him, recognised him afterwards when they met him in a crowded railway station.[2]

2. *Tactile.*—(*a*) During Colonel Taylor's tenancy two ladies and a maid were disturbed by sensations as of moving the bed or handling the bedclothes. This form of phenomenon was much more in evidence on previous occasions, when visitors testified to the beds being violently shaken and the bedclothes being lifted off and thrown on the floor. (*b*) Miss "Duff" on one occasion had a sensation as of struggling with something unseen; and a feeling of an incumbent weight was experienced by her and by another lady. (*c*) The sensation of being pushed by a dog was experienced in two different rooms by Miss Freer and Miss Moore, and had been recorded by earlier witnesses. (*d*) The sensation of a chill was experienced by two gentlemen visitors.

3. The audile phenomena were the most frequent and various. They consisted chiefly of (*a*) rapping on furniture and against walls, crashing and clanging sounds, detonations or explosive sounds, and thuds as from the falling of heavy bodies against the doors. (*b*) Sounds as of voices reading or in conversation. This was a very common occurrence and was testified to by many visitors. (*c*) Sounds as of shuffling footsteps heard in rooms which were empty and locked up, and also by occupants of rooms even in broad daylight. These, too, were testi-

[1] Animals are credited with being sensitive to ghostly hallucinations. The case of Balaam's ass must occur to the reader, and many cases are reported in which horses and dogs have shown great terror at the same time as human beings accompanying them have seen apparitions.

[2] The possibility of an agent evoking an hallucination or "thought image" is dealt with in the report of the Committee on the Census of Hallucinations, published in vol x. of the Proceedings of the Society for Psychical Research (Aug. 1894). The theory is that strong concentration of thought on any object may evoke a corresponding thought in a receptive person, which may be so vivid as to give rise to a sensory hallucination. The mind is supposed to be especially receptive of such thought images during sleep and hypnosis, and numerous experiments are given in the report referred to, in which hallucinations have been intentionally produced in this way. But in this case there is no suggestion of intention. Father H. was greatly exercised in his mind regarding his experiences at B. House, and it may be supposed that his thinking strongly on the subject at the moment when Miss Langton's brain was receptive to etheric thought-waves of corresponding character evoked his image. This point is fully dealt with by Sir William Crookes in his Presidential address to the Society for Psychical Research, 1897.

fied to by several visitors, and by many persons previous to Colonel Taylor's tenancy.

Daytime phenomena occurred upon eighteen days, and night phenomena are recorded upon thirty-five nights during the period of sixty-nine days covered by the journal.

An important feature of the journal is the account of the very great pains which were almost daily taken in attempting to reproduce sounds by normal means, and in testing every sort of theory as to the sources of the disturbances.

The Editors' remark that if expectant attention were the explanation of the phenomena the apparition of Major S. should have been seen, for he was supposed to be principally concerned with the "haunting," and his "ghost" was frequently seen by previous occupants of the house.

A somewhat detailed account is given of this experience in a "haunted house," because it is typical of a large class of similar instances, and it has the advantage over most histories of this nature of being carefully recorded, edited and well written, and of being testified to by between thirty and forty witnesses. To many the whole case will appear trivial and not worth the trouble taken. Some will say, like Professor Huxley, that they don't believe in the genuineness of the phenomena, and if they did, they wouldn't interest them. Others think it is a crushing retort to comment on the uselessness of the results achieved. "If there is a psychic force," they say, "why doesn't it drive a motor or give information from the seat of war?" Mr Andrew Lang justly observes that the telephone is no less wonderful when it only carries the silly tittle-tattle of idle youths; and electricity was no less a force when its only apparent manifestation was the attractive power acquired by amber when it was rubbed. It is not the volume or the market value of a force which should interest the scientific inquirer but its existence.

This is the main aim of the Society for Psychical Research which numbers among its active members some of the leading men of the day, including the Right Hon. Arthur Balfour, M.P., F.R.S., Sir W. Crookes, F.R.S., Prof. O. Lodge, F.R.S., Lord Rayleigh, F.R.S., Prof. Sidgwick, The Marquis of Bute, K.T., and many others of distinguished position. The Society does a great deal of work and publishes elaborate reports of its proceedings. It was only founded in 1882, and it has already given a new turn to psychical studies. Its two volumes of "Phantasms of the Living," published in 1886, and the "Census of Hallucinations," it conducted from 1889 to 1894, are monuments of its work.

In 1894 [illegible] presidential address to the Society in 1894, and spoke [illegible] of which the Society has

done so much to establish, "If it be true, or if anything like it be true, no event, however startling, which easily finds its appropriate niche in the structure of physical sciences ought to excite half so much intellectual curiosity as this dull and at first sight common-place phenomenon."

The reality of telepathy or power to communicate unspoken thought having been accepted, I venture to think it is being used for more than it is worth by some of its exponents. Mr Frank Podmore, for instance, one of the original honorary secretaries of the Society for Psychical Research, published in 1894 an admirable resumé of the work of the Society which he calls "*Apparitions and Thought-Transference: an Examination of the Evidence for Telepathy.*" In this book he endeavours to explain all kinds of psychic phenomena by this means. For example, he supposes that in a haunted house the initial impulse may arise in the mind of A, who, from biliousness, cerebral congestion, or association of ideas, may conjure up the vision of an old woman in grey or a priest with a crucifix. His thought image may affect B, who visits the spot, and through him may be transferred to D, E, and any number of other persons.

I confess that the explanation generally given by spiritualists and savages that the haunting is due to the unquiet spirit of a dead person is more satisfying than this, which also entirely fails to account for the production of material phenomena such as movements of objects which figure so largely in stories of hauntings.

Mr Podmore says (*loc. cit.*, p. 272): "There is at present no trustworthy evidence that an apparition has been weighed or photographed, or submitted to spectroscopic or chemical analysis," and "that the collective perception of an apparition proves the objective or material existence of the thing perceived is probably held now by few persons outside the ranks of professed mystics."

As Mr Lang says, "hallucinations cannot draw curtains, or open doors, or pick up books, or tuck in bedclothes; so if the stories told of such phenomena are true, then some apparitions are ghosts, real objective entities filling space"[1] (*Op. cit.*, p. 206).

[1] Of course it is open to the critic to retort that if a person is so mentally affected as to have an hallucination of sight or hearing so that he thinks he sees an apparition, his sense-deception may be carried a little further, and he may imagine the curtains or bedclothes move or doors are opened without hands, though nothing of the kind has happened. This sense deception can, as we know, be brought about by suggestion in hypnotised subjects, and it is said to form the basis of much eastern magic, one definition of a magician being "a person who can make people see things as they are not." The cautious observer will probably decline to accept either the animistic conception of the spiritualists or the telepathic theory of Mr Podmore, but will prefer to wait for further evidence before committing himself.

Some of the visitors to B. House were provided with scientific instruments, and it was intended to attempt to secure phonographic reproductions of the noises and seismographic measurements of the vibrations, but the owners of the property absolutely refused to prolong the tenancy or allow of further investigation; but no future inquirer will be considered competent unless he provide himself with the latest scientific arms of precision.

The editors say (p. 36), "A haunted house (or any other locality) is merely a place where experience shows that hallucinations are more or less localised, and the only especially interesting question about it is why the hallucinations should be localised at a particular place, and what causes them there."

Mr Andrew Lang has investigated the subject from the point of view of a folk-lorist and anthropologist, and in his book, "Cock Lane and Common Sense," gives the results of his researches. He shows how old are these ideas, and quotes the case of an Egyptian widower whose house was so haunted by the ghost of his deceased wife that he petitioned the gods to give him rest, and his prayer, written on a piece of papyrus 6000 years old, was found in a temple by M. Massero (p. 2). Not only in the ancient civilisation in Egypt, but in that of China of to-day, the spirits of the dead are a source of anxiety to the living, so that no threat is more terrible to a Chinese than that his enemy will kill himself and send his spirit to haunt him. Throughout the barbarous parts of the world, whether in West Africa, the Australian bush, or the islands of the Pacific, we find the same stories cropping up. As M. Littré said, "It is always the same old story." This he thought was a condemnation, but to many it will prove an attraction. The object in view is to get to the rational explanation of these wide-spread beliefs and phenomena.[1]

Of course there is an immense deal of trickery and self-deception, and this is one reason why many would-be inquirers are repelled. Most of us have heard of haunted houses from persons who say they have met people who have been in them, but few of us are able to trace such stories to their source. The often-told tale of the haunted house in Berkeley Square, now

[1] It may be said that the universal prevalence of an idea does not prove its truth, and the belief in witchcraft may be instanced as an example. But recent researches by Charcot and others at the Salpêtrière have demonstrated the reality of certain of the most commonly reported proofs of satanic possession. One proof upon which great reliance was placed was the absence of pain on pricking, which was a sure sign of guilt. Cutaneous anæsthesia or analgesia is now recognised as one of the characteristic features of hysteria, and many poor hysterical women have been sent to the stake from the misinterpretation of this symptom.

pulled down, is a case in point. Many so-called haunted houses owe their character to stories set afloat by hysterical women or children, and often the phenomena are traced to mischievous persons who desire to create a sensation. Frequently mysterious noises are found to proceed from such simple natural causes as the movements of rats or the creaking of boards. Medical men are well acquainted with conditions in which patients are haunted by apparitions of snakes and other disagreeable visitants. A brisk purge has laid many a ghost, and a walk on the cliffs or a game of golf may be recommended to many a seer. Mr Lang, however, says (*loc. cit.*, p. 151), as far as his experience of first hand tales is concerned, the persons known to him who say they have seen ghosts in haunted houses were neither unhealthy nor, except in one solitary case, imaginative persons, nor were they *expecting* a ghost.

But after making due allowance for fraud, self-deception, mal-observation, and ill-health there remains a residuum of instances in which phenomena occur which are inexplicable in the present state of our knowledge.

Recent discoveries of an unexpected nature, such as those connected with the Röntgen rays and wireless telegraphy, may make men of science more ready to investigate forces which are only "supernatural" because unexplained.

Sir William Crookes said recently, "All the phenomena of the universe are presumably in some way continuous, and it is unscientific to call in the aid of mysterious agencies when with every advance in knowledge it is shown that ether vibrations have powers and attributes abundantly equal to any demand—even to the transmission of thought."

C. L. T.

Meetings of Societies

Edinburgh Medico-Chirurgical Society

THE second meeting of the session was held on Wednesday, 6th December—Mr A. G. Miller, the President, in the chair.

Mr Wallace showed a patient after pylorectomy by Kocher's method; also the following specimens:—calculus in ureter, fractured skull, calcareous mass from cerebellum, tumour of pituitary body and tumour removed in case of pylorectomy.

Mr Stiles exhibited a child after operation for severe deformity following burn of ankle, a child with ventral hernia after operation for suppurative appendicitis, and an infant after

operation for intussusception, which was found to be secondary to acute appendicitis.

Dr Skirving showed a patient after removal of gangrenous appendix with general peritonitis, a patient after subperiosteal resection of lower jaw.

Mr Miles showed an enchondroma of femur, two cases of excision of the cæcum, and cases of fracture of the base of the skull.

Dr Harvey Littlejohn showed a dermoid cyst of the ovary, and a heart with calcareous degeneration.

Dr A. Douglas Webster showed a hot-air spray; an account of which will appear in a future number of the Journal.

Dr Gulland gave a microscopical demonstration of Cohn's method of showing fat in urinary deposits and other fluids, which replaces the unsatisfactory osmic acid method.

Dr C. W. Macgillivray read a paper on the radical cure of hernia, with the statistics of seven years' experience in the Royal Infirmary. He said that only three forms had to be considered: inguinal, generally in the male, rarely in the female; femoral, commonly in the female, not unfrequently in the male; umbilical, practically confined to infants and middle-aged or old women, and seldom calling for operation except in the case of the latter. The cases suitable for radical cure were divided into those in which operation comes to be either imperative, necessary, advisable, or convenient.

1. *Imperative.*—The cases of hernia in which operative treatment becomes imperative, are those in which there is strangulation or some other complication, and in which, if no operation were performed, death would inevitably result. In such the question of radical cure is only secondary, and whether or not it is justifiable to attempt the operation for radical cure depends upon the condition of the parts and the state, age, and circumstances of the patient.

2. *Necessary.*—Sometimes operation is necessary, although not imperative. Cases which come into this category are those in which the hernia cannot be kept up by means of a truss; this makes the condition of the patient one of constant danger, accompanied by pain and inconvenience, and commonly renders him unfit for his ordinary work.

3. *Advisable.*—Less urgent than either of the preceding groups are cases in which, although operation is by no means necessary, it may still be, and ought to be, recommended. These are found, for the most part, amongst patients of the poorer class who have to work hard. They are cases in which a truss would be amply sufficient for a patient leading a seden-

tary life or a life of ease, but in which, during the severe exertion of body to which poorer patients are often exposed at their work, the hernia would not be sufficiently controlled for safety. With these may be included those congenital hernias in which the poverty of the patient's circumstances prevents the acquisition in the first place of a suitable truss, and in which, even if a proper truss were used, the constant care of it which is necessary to the production of a permanent cure could not be guaranteed.

4. *Convenience.*—Where a truss acts quite satisfactorily, but the patient does not care to put up with the inconvenience of any apparatus and wishes to be permanently relieved, even at some slight risk, an operation of convenience may be performed. And in this group are cases also in which the presence of a hernia, however slight it may be, proves a bar to congenial occupation or to the public services.

The age of the patient naturally influences the danger and the mortality. Youth, even infancy, does not appear to add to the danger. The mortality of uncomplicated cases in young subjects is practically nil. After middle life the danger increases, and in old age it is considerable, so that the operation should not then be undertaken rashly, especially if the patient's general health be not good or if the hernia be large or complicated.

But quite apart from the immediate danger, the ultimate result and its permanency require consideration, and the important prognostic points are the age of the patient, the method employed, the nature of the rupture and of the surrounding parts, the mode of healing and the after-treatment. The earlier the operation the better, because with the growth of the patient there is a natural tendency to cure. With regard to the method in children and imperative cases, the simpler the better. In the young and healthy more thorough and more complicated methods may be employed.

The treatment of the sac which Dr Macgillivray usually employs, consists in clearing it from the constituents of the cord as far up as the level of the internal ring, transfixing and tying it with chromic catgut. The long ends of the catgut are then passed by means of Macewen's needles, the one end through the lower edges of the transversalis and internal oblique, and the aponeurosis of the external oblique, about an inch to the outer side of the internal ring; the other, at the same level, through Poupart's ligament. When the two ends are brought together and tied, they not only draw the stump of the neck of the sac up towards the anterior superior iliac spine, but also strengthen the outer half of the circumference of the internal ring.

As to closure of the canal in infants and in cases of strangulation, Dr Macgillivray does not recommend the splitting up of the anterior wall. In the former a few sutures of chromic catgut passed through the aponeurosis of the external oblique, catching up the lower edges of the internal oblique and transversalis and passing in front of the cord protected by the finger, out through Poupart's ligament, do all that is necessary. In the latter the same method may be employed, but in severe cases and in old people the canal had better be left alone to contract of itself.

In young healthy individuals, or where the canal is large, a modification of Bassini's method may usefully be employed.

In femoral hernia the closure of the canal is notoriously difficult. One or two chromic gut sutures are passed through the fascia over the pectineus and Poupart's ligament so as to close the greater part of the canal at the inner side. Sutures cannot be safely introduced towards the outer side on account of the proximity of the femoral vein.

Dr Macgillivray believes that healing by first intention is of the utmost importance and that failure in this respect is one of the most frequent causes of subsequent relapse.

A point of great importance in the after-treatment is keeping the patient in bed in the horizontal position for six weeks after the operation so that the wound may become firm and less liable to stretch. When everything has gone well, a truss is unnecessary except during violent efforts.

During the past seven years Dr Macgillivray performed 91 operations for radical cure with 10 deaths. 8 of these deaths occurred in cases of strangulated hernia in which the prognosis was bad before operation, and in which the question of radical cure had nothing to do with the unfortunate result.

Of the 91 patients, 70 suffered from inguinal hernia, 17 from femoral, and 4 from umbilical or ventral hernia.

Inguinal hernia, 70 cases with 6 deaths, 4 of the fatal cases being complicated by strangulation.

Femoral hernia, 17 cases with 3 deaths, all females, aged 34, 64, and 66, and all cases of strangulation.

Umbilical and ventral hernia, 4 cases and 1 death.

These results would show a death-rate of a little over 3 per cent. in all the cases, favourable and unfavourable, which were operated on for radical cure simply. In the 31 cases complicated by strangulation, or a similar severe condition, the total number of deaths was 8, or about 25 per cent.; in each case the condition before operation was amply sufficient to account for death.

Remarks were made by Messrs Cathcart, Alexis Thomson, Stiles, Cotterill, and the President.

Edinburgh Obstetrical Society

THE second ordinary meeting was held on December 13th, Dr Milne Murray in the chair. Among the specimens shown were a double uterus with a cystic ovary, by the President; a large fibroid removed by supra-vaginal hysterectomy, by Prof. Simpson; a large foreign body from the female bladder, by Dr Lamond Lackie, and an unusually large cyst of Bartholin's Gland, by Dr Haig Ferguson.

Prof. Kynoch of Dundee gave an interesting communication on *vaginal coeliotomy* with illustrative cases. After referring to the history and technique of the operation, he dealt with the indications in detail. For *retroflexion of the uterus* he had employed Dührssen's operation successfully in two cases. The pain and dysmenorrhœa were relieved and the uterus retained its normal position. Pregnancy has not as yet occurred since the operation.

In *inversion of the uterus* a posterior colpotomy might be successful as in the case reported by Dr Berry Hart. Satisfactory results may be obtained in *chronic ovaritis with adhesions* by breaking down the adhesions through either an anterior or a posterior colpotomy. He reported three such cases where the usual symptoms of long standing had been relieved. When palliative treatment has failed in *prolapse of one ovary* removal of the organ by posterior colpotomy is justifiable. Two such cases did very well, the patients being subsequently able to wear a pessary with comfort for coincident uterine displacement. Two cases of *pelvic hæmatocele* arising from early intra-uterine gestation treated by posterior colpotomy were able to leave hospital ten days after operation —pain and physical signs being both removed. In the one case a normal pregnancy and confinement succeeded.

Three cases of small *ovarian cysts*, including a dermoid and a papillomatous cyst, made good recoveries after removal through a posterior colpotomy. Two of them left hospital on the fourteenth day. In cases of *diseased appendages* with adhesions there is a distinct danger of wounding the viscera, especially the bladder and rectum, when operating by this route. He reported a very successful case of *pyo-salpinx* drained through the vagina, the patient at the time of operation having a temperature of 104° and a pulse of 136. Entire relief from pain followed and menstruation became regular and painless. A case of *hydro-salpinx* thus treated did equally well. He had seen no case of myoma justifying this method of removal. Among other indications for the

use of colpotomy, Dührssen suggests that in cases of phthisis, cardiac affections, and other conditions likely to be aggravated by pregnancy, ligature and division of the tubes should be performed by the anterior route. For diagnostic purposes the vaginal route may be used to ascertain the amount of glandular infection in cancer of the cervix; it may also be used for treatment in cases of perforation of the uterus and tubercular ascites. The advantages of the vaginal route are: shorter convalescence, no abdominal cicatrix, less shock, and better drainage; the only drawbacks being the limited space and the greater difficulty of rendering the vagina aseptic. Prof. Kynoch gave the following cases as the most suitable for treatment by colpotomy, other means having failed:—(1) Mobile retroflexion, causing symptoms at or about the menopause; (2) chronic ovaritis, with adhesions; (3) prolapse of the ovary, with fixation; (4) ovarian cysts, small, whether dermoid, simple or papillomatous; (5) pelvic hæmatocele; (6) unilateral mobile tubal swellings. Unsuitable cases are: large tumours and old-standing tubal disease, with dense adhesions, when complete removal of the appendages is necessary. The paper was freely discussed by the fellows present.

Notes of a case of *pyo-salpinx removed by laparotomy in the early puerperium* were read by Dr Milne Murray. The diagnosis of the condition in early pregnancy is comparatively easy, but in the later months almost impossible, and the first hint of trouble may come from the rupture of the tube or the oozing of its contents through the abdominal opening setting up peritonitis. The patient, æt. 35, had had one child two and a half years before. There was a history of tubal mischief on the left side. Dr Murray saw her on July 12th. She was then approaching her full time. For some months pain in varying degrees had been present on the left side; on the 7th it suddenly became excruciating, and remained troublesome for the next five days, with a temperature of about 101°. She lay on her back with the knees drawn up, her expression was anxious, and her complexion of a yellowish tinge. Gentle pressure in the left ovarian region caused wincing; but *per vaginam* nothing abnormal was to be felt; the cervix was quite taken up and the membranes bulged slightly. Concluding that there was tubal mischief present, the question of operation or a hurried delivery was discussed and abandoned. Labour set in and was concluded during the night of the 13th. For the next two days she was somewhat prostrate, with temperature and pulse both up. On the morning of the 15th she became suddenly worse, temperature 103·5, pulse 130, with all the symptoms of an acute peritonitis. The lochia was quite sweet. Tenderness and fulness were made out in

the left fornix. Dr Murray decided on immediate operation. On opening the abdomen, the pelvic peritoneum was found covered with plastic lymph. The left tube and ovary were intensely congested. The broad ligament was ligatured close to the uterus, and tube and ovary removed. The abdomen was washed out and closed without a drain—about a pint of lysol solution being left in the cavity. The tube on examination was found to contain a small quantity of pus. The patient made an uninterrupted recovery.

Dr Simla Paterson communicated a case of *rapid death following labour, due to cardiac disease.* He first saw the patient at midnight on November 5th. She was 32 years of age, and had had one previous child. Her confinement was due about the middle of November. He found her in a state of extreme breathlessness which had come on suddenly four hours previously, with much coughing and expectoration of blood mixed with mucus. Two similar though less severe attacks had been experienced during the pregnancy. There was comparative dulness of both bases, and coarse crepitations all over the chest. The tumultuous action of the heart hid any murmurs. Respiration, pulse, and temperature were all raised. There was no œdema present, and labour had not commenced. Returning at eight o'clock next morning he found child and placenta born, and the patient in a state of extreme dyspnœa. With Dr Haultain in consultation he came to a diagnosis of pulmonary thrombrosis. Death occurred at 8 P.M., exactly twenty-four hours after the onset of symptoms. There was little in the previous history to help one, she had suffered from chorea. A *post-mortem* examination showed acute pneumonia in both lungs, marked mitral and aortic stenosis. The left kidney was completely cystic, and contained a quantity of tubercular pus, while the right showed serious cirrhotic changes. These serious conditions had apparently given rise to little or no discomfort during the pregnancy.

Glasgow Medico-Chirurgical Society

AT a meeting on Friday, November 17th,

1. Dr M'Call Anderson showed a patient suffering from disease of the spinal cord with special reference to ætiology and treatment. He was a tailor, aged 27, without history of specific disease or intemperance. For some years he had had difficulty in walking, and had had symptoms at times suggestive of implication of the posterior columns of the cord, such

as numbness in the legs, and girdle sensation, numbness in the left arm, and attacks of giddiness and staggering, and nausea. There had been constipation, but no bladder trouble.

When first seen by Dr M'Call Anderson, ataxia was the most prominent feature. His gait was typical, "stamping" action being marked. The tendon reflexes were found to be much exaggerated, and the superficial reflexes very pronounced. He had felt a scalding sensation at the tip and side of the tongue. When shown to the Society, the patient was almost cured. He could walk fast and steadily, although a slight degree of stamping was still present; the reflexes were nearly normal; he had no numb feelings, or pain in his tongue. It was observed that his nose was rather sunken, his forehead prominent, and at one time he had had a pallid colour, suggestive, with his general appearance and shape of nose, of hereditary specific disease. There was no history of miscarriages before his birth, but, notwithstanding this, he was treated by large doses of iodide of potassium, beginning with 10 grains thrice daily and increasing up to 120 thrice daily. During the treatment the patient gained six pounds in weight, and very great improvement in his condition occurred.

Dr Anderson had in other cases given as much as 600 grains of iodide of potassium daily. He remarked that small doses seemed to produce iodism more readily than large ones. He did not consider that the results of treatment proved the specific etiology of the disease, but they strongly suggested it.

Dr Alex. Robertson asked if there were any indications in the eyes or teeth, and was answered in the negative. He considered the case to be one of hereditary specific disease, which, in the spinal cord, occurred often as an irregular form of disease, not affecting one column only. He had often noticed the benefit which large doses of iodide brought about, say 40 grains or more, when less doses had failed to do good.

Dr Dunn had had the case under his care for Dr Anderson during some weeks, and confirmed the presence of marked ataxia and other symptoms, and this great improvement under the large doses of iodide. In psoriasis and other skin complaints he had also seen these large doses of iodide do good.

Dr Kennedy had some doubt whether really the large doses were proportionately beneficial. Much of the drug must be rapidly excreted by the urine. He had seen ataxic symptoms and specific gluteal ulcer relieved in a surprisingly short time by doses of iodide of potassium of 5 grains thrice daily.

Dr M'Call Anderson believed in the large doses, and though

no doubt the drug rapidly passed out in the urine, still whilst in the tissues it was able to do good, and the more of it apparently the better in these cases.

(*b*) A female, aged 30, suffering from the spleno-medullary form of leucocythæmia. A year ago she became pale, lost flesh, had palpitation, and a swelling appeared in the left side of the abdomen. She was now emaciated and very weak and anæmic, with an enormous enlargement of the spleen and slight enlargement of the liver. There was no enlargement of the lymphatic glands. There were a few small hæmorrhages in the retinæ. There was an enormous increase of leucocytes in the blood, chiefly of the polymorphous nuclear cells, and many myelocytes. The lymphocytes were not increased. There was an average of one white cell to every six red. The red cells numbered 2,720,000, and the hæmoglobin was 45 per cent. of the normal amount.

She had been treated so far by raw red bone marrow, half an ounce thrice daily, and by 5 grain tabloids of splenic extract, but up to now no good result had occurred.

Dr Alex. Robertson, after remarking on the varieties of leucocythæmia, and the diagnosis of this case as being of the spleno-medullary form, uttered a caution against operative procedure of any kind, and in this view Dr Dunn expressed entire agreement.

Dr Hunter said he was specially interested in the case. In a similar case he had noted many micro-organisms in the blood, and in literature he had read of two other observations of this nature—one of these in the last number of the "Edinburgh Hospital Reports." He had not, however, been able to make a culture of the organism, and could not say whether it was saprophytic or pathogenic.

He considered that the enlargement of the liver was probably due to the deposit of lymphatic tissue in the organs, which was known to occur in leukæmia.

Dr Clark said the question of operation was determined by that of the origin of the disease. If it originated in the spleen and bone marrow, then, of course, no surgical procedure would be thought of.

Dr M'Call Anderson, in reply, agreed with Dr Hunter that the liver was increased in size from infiltration with lymphatic tissue. In lymphadenoma the same thing occurred with increase of white cells in the blood. With regard to treatment by arsenic, mentioned by Dr Robertson, he had not found it do much good in leukæmia, not so much as in pernicious anæmia. He had not used it, as he desired in this case to watch the effects of the spleen extract and bone marrow alone. As to

operation, the grave objection was that the patients usually died from hæmorrhage.

2. Dr Hinshelwood showed a case of tabes in the pre-ataxic stage, with characteristic eye-symptoms, and referred to the importance of early eye-symptoms in tabes, as such cases were often put down as "rheumatic" when lightning pains were present, or as dyspeptic when there were gastric crises.

In this case the patient had complained of double vision, and there was paralysis of the third nerve, ptosis, etc. The Argyll-Robertson phenomenon was present, and also Westphal's sign, which consisted in the absence of the normal pupillary contraction that occurs if the orbicularis be contracted, while closure of the lids is prevented by the hand of the observer.

He also found optic atrophy, although the patient had made no complaint of defective vision. The visual field showed a well-marked centric contraction. Four months previously, with those eye-symptoms present, the knee jerks were not absent, but Dr Hinshelwood had predicted their disappearance, and this had occurred. With regard to prognosis, cases with early eye-symptoms usually showed very slow progress of the spinal ones, but the optic nerves did not improve, but tended gradually to get worse.

Dr Dunn had hoped to hear that good use of the early diagnosis had been made in the treatment.

In reply to this Dr Hinshelwood said that though there was no history of syphilis there was a history of a chancre, and specific treatment had been instituted at once. There had ensued improvement in the ocular palsy, but it had to be remembered that these early ocular palsies frequently improved or passed away without treatment, and as the spinal symptoms often progress very gradually, there had to be caution in ascribing any improvement, or seeming check in the malady, to the treatment. Of course early specific treatment must have far more chance than late.

In reply to Dr Ness and Dr Clark he added that Westphal's sign was not peculiar to locomotor ataxia, as it was found in general paralysis of the insane, and other nervous diseases. He had observed it in every case of tabes, where also there had been present the Argyll-Robertson phenomenon.

3. Dr J. Soutar M'Kendrick then read a paper on a "case of epithelioma of the œsophagus involving partially the pneumogastric nerve with its recurrent branch on the right side, and where the symptoms closely pointed to aneurism of the arch of the aorta."

At a meeting on December 1st,

1. Dr Middleton showed a man suffering from well-marked

myoclonus multiplex. The muscles chiefly affected were the extensors and adductors of the thigh, the recti abdominis, the gluteal muscles, the erector muscles of the back, the shoulder muscles, and the masseters, but mostly those first mentioned. The muscular contractions were very powerful, and the members of the Society had an excellent opportunity of seeing this rare condition. The contractions were unceasing. Dr Middleton said they were diminished, but did not cease, during sleep. They varied from twenty to sixty per minute. The patient had suffered for some years, and was now quite unfitted for work, and mentally melancholic. During voluntary movement, however, they were controlled. The patient, in turning round on the couch, was nearly jerked off by the vigour of the contractions in the thigh muscles. There was exaggerated knee reflex and ankle clonus.

The case was obviously not one of chorea, nor of the condition known as electric chorea, for in this latter disease there was wasting and palsy of muscles, which was not present in the patient shown, and, moreover, cases of electric chorea were usually fatal in a few months. Dr Middleton considered that the case must be held to be one of myoclonus, or para-myoclonus multiplex, which simply expressed the fact that a great many muscles and group of muscles were affected by clonic spasm. It corresponded to Liebreich's description of cases observed by him. The pathology of the condition was unknown. The patient had had a bubo, and said he had had syphilis, and believed that this was the cause of his trouble. Iodides and bromides of potassium, chloral, the constant current, had been tried without benefit.

Dr Alex. Robertson agreed with the diagnosis of this rare case. In chorea there were no rhythmic contractions, such as were present in this patient. He was inclined to think that it was of the nature of hysteria. The mental condition might be viewed as an hysterical stigma. He thought the use of the interrupted current might do good.

Dr Scott inquired as to any history of a mental shock or fright. He had had under his care a man who, after being the accidental cause of the death of a child, became choreic in his actions, and the muscular movements were somewhat similar to those of the patient just shown.

Dr Clark thought there might be some secondary change in the spinal centres.

In reply to Dr Rutherford, Dr Middleton said the movements did not cause fatigue. Their continuity was surprising.

2. Dr Freeland Fergus related the main facts of a case of unusual ocular paralysis. The patient was a young lady, aged

seventeen years, seen in private, and since birth (such, at least was the history), she had been unable to move her left eye in any direction to any appreciable degree. The right eye moved normally. In the left eye there was neither ptosis nor iridoplegia, notwithstanding the paralysis of the other muscles. There was no exophthalmos, or anything in the orbit to account for the fixation of the globe. Dr Fergus inclined to think that there must be some central lesion, but he had recounted the case with a view to eliciting the opinion of the Society upon it.

Dr A. Robertson suggested that some injury at birth affecting the nerves at the base of the skull, the 3rd, 4th, and 6th, might be the cause, though it would be necessary to explain the absence of ptosis in some other way.

Dr Fergus replied that there was no such history, and that the labour had been normal and easy.

Dr Munro was reminded by the case just related, of some cases described by Sir W. Gull. The idea was that the nerves were affected just as they entered the muscles, by a form of neuritis, possibly rheumatic. If the ciliary ganglion escaped then the action of the pupil would not be affected. Considering the normal state of the pupil it was difficult to entertain the idea that the third nerve was involved at the base of the brain.

Dr Leslie Buchanan suggested the possibility of the congenital absence of some of the muscles. Such cases had been recorded.

Dr E. Thompson asked if the action of the internal rectus of the right eye was normal, and being answered in the affirmative, said that that was against the theory of the lesion being central.

3. Mr R. H. Parry showed two specimens of malignant stricture of the large intestine.

(*a*) The first specimen was the greater part of the sigmoid removed from a young woman aged 24, who had been seemingly in good health till within four days of operation, when her medical attendant had diagnosed obstruction of the bowels, and sent her in to the Victoria Infirmary. There was abdominal pain and distension, vomiting, of fæcal material latterly, and obstruction, with normal temperature. Mr Parry followed his usual procedure, and opened the abdomen in the middle line, flushed out the cavity with saline solution, and began a systematic examination of the bowel, starting with the rectum. He found a mass in the sigmoid. He made no attempt to excise this, but made a colotomy incision in the right inguinal region, and opened the bowel there at once, introducing a piece of ordinary drainage tube into it. Previous to opening the bowel he had closed the median incision. The patient was very well next day, and got on capitally. Six weeks later he operated

on the growth in the sigmoid, removing it, and joining the cut ends of the bowel by the use of a Murphy's button. With regard to the use of aids, such as the button, Mr Parry said he was guided by circumstances. If there was time, that is if the patient's condition allowed it, he stitched bowel to bowel. If the patient's state demanded haste, he used the button or other similar device. The patient did very well. The wound in the right groin was very readily made to close after the mucous membrane had been clipped away and a little caustic applied. He found the incision in the right groin very useful, even in rectal cases, and advocated it.

Dr Buchanan who had examined the specimen had suggested that it might be inflammatory. It showed a very tight stricture, and the mucous membrane seemed healthy up to the seat of stricture. Mr Parry, however, still thought it was malignant, despite the age of the patient. He had seen malignant disease in the bowel of this nature, at a younger age. It was extraordinary that so tight a stricture should have existed while the patient remained in good health till within a few days of total obstruction.

(*b*) The second patient was a female aged 56, who had been ill three weeks before coming to the hospital. She was admitted suffering from acute obstruction of the bowels. The same treatment was carried out, median incision, flushing of cavity of abdomen with normal solution, and systematic examination of the bowel beginning at the rectum. Nothing was found in the rectum or sigmoid, but the cæcum was felt to be distended. So taking into consideration the age of the patient, and the shock attendant upon prolonged exploration, he made an incision in the right groin, drew the cæcum into the wound, and opened it at once, after closing the median incision. He stitched the cæcum to the skin, thus establishing a permanent fistula. Somewhat later the condition of the patient became so much improved that he attempted the radical cure, and opened the abdomen above the umbilicus. A tumour was found at the hepatic flexure which he excised, and from difficulties with the joining of the ends of the bowel, he was compelled to use Murphy's button. It was passed three weeks later. Ultimately the colotomy wound was closed by clipping the mucous membrane and touching it with caustic. The result in this case, from first to last, was extremely satisfactory.

Mr Parry then made a few general remarks on obstruction of the bowels. The most important thing was to find out whether the obstruction was in the large or the small intestine. If there was obstruction in the large bowel then a palliative operation might be done, as a first stage (colotomy), and a

radical cure attempted later on. But if the obstruction were in the small intestine the case as a rule must be treated to a conclusion at one operation. The observance of this distinction undoubtedly tended greatly to reduce the mortality in operations for obstruction of the bowels. Mr Parry considered it more important to teach students the distinction between obstruction in the large and in the small bowel, from this point of view, than it was to teach them to diagnose between, say, a volvulus and an intussusception. In former days in cases of obstruction the abdomen was opened, the bowels turned out, and the site of obstruction looked for, and the condition then dealt with, and the mortality was terrible.

Dr Rutherford agreed as to the importance of the diagnosis between small and large bowel obstruction, but supposed Mr Parry intended that the diagnosis was completed after the abdomen was opened. That operations on the large bowels should, in cases of obstruction, be divided into two stages, was a valuable point, and did not require exemplification from him. The surgeon's first duty was to deliver the patient from death impending from the obstruction. He did not however admit that in cases of obstruction of the small bowel, the surgeon was bound to complete the whole treatment of the case at one operation. With regard to the question of age, it was not so rare as might be considered to meet with malignant tumours of the bowel at an early age. He had seen a case of rectal cancer in a girl about twelve years old.

Mr Clark congratulated Mr Parry on the results of the two cases. The prognosis in cases of acute obstruction was certainly much more favourable to-day than it was some years ago. It was certainly proper treatment first to relieve the patient of the obstruction, and later to deal with the disease causing it, when such delay was at all possible. Mr Parry's choice of the right side for colotomy was somewhat novel, though Mr Clark had done this also for many years in suitable cases.

Meeting on Friday, 15th December.

1. Dr Robert Kennedy showed (*a*) Two patients who were operated on for appendicitis, in one of whom general peritonitis was present; (*b*) a man seventeen months after excision of the entire lower lip for epithelioma, with formation of a new lip by Trendelenburg's method; (*c*) a case of gumma of the mamma.

2. Dr Henry Rutherford showed a patient with deformity of abdominal wall following resection of ribs for empyema.

3. Dr J. Henderson Nicoll showed (*a*) an infant in whose case congenital obstruction of the pylorus was diagnosed (by Dr John Ritchie) and relieved by operation; (*b*) a perforating

gastric ulcer, successfully excised six days after perforation; (*c*) a ruptured tubal gestation removed by laparotomy; (*d*) calculi from three prostatic cases in which the calculous formation was secondary to and marked by the symptoms of the prostatic hypertrophy; (*e*) a case of congenital absence of a number of bones in hands and feet.

4. Dr Hunter showed for Dr Lindsay Steven and Mr Luke a patient who had been trephined on account of right-sided hemiplegia, in whom a large hæmorrhagic cyst over, or on, the left cerebral hemisphere was evacuated, and who recovered his speech after the operation.

Aberdeen Medico-Chirurgical Society

A MEETING of this Society was held on December 7th—Dr W. H. Williamson, President, in the chair.

Drs W. Christie, W. Crombie, Noble, Philip, and Westland were elected resident members; and Drs Arthur, Cults, and Elliott, Walthamstow, non-resident members.

Dr J. L. Salmond exhibited and read notes on a case of incipient tabes dorsalis in a female. Patient, unmarried, age thirty-seven, no nervous family history, nothing pointing to syphilis or alcoholism, always healthy up to present illness, which she attributes to "nursing an old man who was in a filthy state." Illness began in autumn of 1898, with languor and aching pains in legs, which were occasionally of a darting, excruciating character; about the same time violent paroxysmal pains in the stomach, described as "like the pains of labour, only in the stomach," accompanied by severe rectal distress and tenesmus, but no vomiting; constipation. Under treatment for the gastric condition the stomach and rectal pains disappeared in a few days and she felt much stronger for a year, but several times during the year she suddenly lost the power of her legs, so that she fell down and was quite helpless for about fifteen minutes. The power came back gradually. She has also had attacks of vertigo, also diplopia when she looked long at an object.

Dr Salmond first saw her in the autumn of this year, when she complained of a recurrence of the symptoms—aches and darting pains in the legs, paroxysmal pains in the stomach, weakness and paræsthesias (*e.g.* numbness, coldness, etc.) in the feet and legs. The ground felt soft beneath her, and she did not feel steady in the dark. Between the shooting pains in the legs, the legs felt "as if they were knocked or bruised," and she says they were tender to touch, but this was never made out.

Condition on Examination.—A spare woman, but no localised wasting or weakness, no tenderness of muscles, electrical reactions of legs normal, knee-jerks absent, plantar reflexes active, no ataxia of gait, but Romberg's symptom well marked ; tactile sensation diminished on feet and legs, pain, temperature and localisation senses normal; no ulnar or peroneal analgesia; pupils unequal, left greater than right, rather dilated, quite circular; both pupils react sluggishly to light and on accommodation ; on throwing light into the eyes the pupils first contract and then slowly dilate almost to their previous condition, the illumination being constant, as though the light reflex was impaired ; ocular movements appear full, but diplopia is readily elicited in various directions ; visual acuity good, both fields peripherally contracted, left optic disc paler than right; no bladder symptoms, albuminuria or glycosuria. Under treatment with arsenic for nearly two months, her general health had improved greatly and many of the symptoms were less marked (*e.g.* pains, sensory disturbances, peculiar reaction of pupils, etc.).

Dr Salmond thought the diagnosis lay between tabes and multiple neuritis. He discussed the various points in favour of each, and made some remarks on the newer views of the pathology of tabes, showing how nearly akin in pathology these two diseases are. He held that the clear history of lightning pains and gastric and rectal crises, vertigo, temporary palsies of the limbs, and especially the incipient optic atrophy and other eye symptoms—special attention being drawn to the peculiar reaction of the pupils, which probably marked an early stage of reflex iridoplegia—pointed with considerable definiteness to a peripheral form of tabes. He pointed out that the nutritional aspect of the ætiology of tabes made prognosis more hopeful and treatment more rational, being directed to stimulating the nutrition of the sensory neurons involved.

Dr Marnoch read notes of a case of strangulated hernia, with unusual contents of the sac. Patient, age sixty-five, was admitted to Aberdeen Royal Infirmary, complaining of swelling and acute pain in the right side of his scrotum. He had been exceedingly healthy all his life, but six years ago, while engaged at some heavy work, he suddenly felt an acute pain in his right groin and fell down. A rupture was found and reduced, and a truss was worn for six months. He then took it off, but the rupture came down again larger than before, and there was much pain. After reduction he again wore his truss, which successfully kept up the hernia for about two years, but then it came down very frequently. Until the present occasion, however, it was always capable of being reduced. On examination he was found to have a right scrotal hernia about the size of a

turnip. Taxis and manipulation were tried for about forty-five minutes without success, the hernia retaining its original bulk and tenseness. Taxis was again tried under an anæsthetic with a similar result. Dr Marnoch was then summoned, and decided to operate. An oblique incision about four inches in length was made, commencing just above the external ring, and the hernial sac was opened. The contents were found to be the cæcum with appendix, a small part of the ascending colon, and a few inches of the ileum. These, after some difficulty, were manipulated back into the abdomen without cutting the already enlarged external ring; the inguinal canal and external ring were narrowed by three silk sutures, a good grip in each case being taken of the conjoined tendon. The recovery of the case was uneventful.

Dr Marnoch referred to the comparative rarity of the cæcum being found in a hernial sac in the groin. Among 417 cases of herniotomy collected by Brunner, the cæcum was found in the sac in 2·8 per cent. He pointed out that the presence of the cæcum and appendix in a hernial sac is to be explained by an arrest in the development of the large bowel at the stage when it has a mesentery, and that the cæcum may enter either a left or a right hernia, according to the stage of development at which the arrest takes place, the cæcum being originally on the left side, and travelling later to the right. There is one other way in which hernia of cæcum and appendix may be accounted for. In an ordinary hernia containing part of the ileum, if more small bowel descends, it may drag the cæcum and ascending colon after it, and this will take place all the more readily if the above-mentioned developmental anomalies are present.

Dr Ashley W. Mackintosh read notes on the recognition of objects by handling (the stereognostic sense). These will appear in a future number of the Journal.

An interesting discussion followed the various papers.

Forfarshire Medical Association

AN ordinary meeting of this Association was held on Friday, 1st December, at 4 P.M., in the University College, Dundee, Dr Templeman, Vice-President, in the chair.

Professor MacEwan gave notes of a case of tracheal stenosis following cut throat. Patient, a man aged forty, cut his throat in October 1896, and a short time after the wound had healed, it was found necessary to perform tracheotomy owing to his gradually increasing difficulty in breathing. He improved, the tube was removed, and he was dismissed from the hospital. Patient

returned some time afterwards complaining of difficulty in breathing again, and it was found necessary to open up the old tracheotomy wound and reintroduce a tube. An attempt was then made to treat the tracheal stenosis by the passage of bougies upwards through the tracheotomy wound. After a time the tracheotomy tube was withdrawn, but it was found that the stricture tended to contract. Professor MacEwan now got an intubation tube made, similar to O'Dwyer's tubes but much longer and broader, because O'Dwyer's largest tube did not reach the stricture. This was daily passed through the stricture, and the patient was then able to do without the tracheotomy tube. By paring the edges and suturing, the tracheotomy wound ultimately closed. It was then found possible to discontinue the passage of the intubation tube, and the patient has now returned to work, having had no difficulty of breathing for the last four months.

Professor MacEwan then made some remarks on the value of intubation in laryngeal diphtheria.

He quoted statistics of his own, from which he considered that in many cases intubation was preferable to tracheotomy. He then discussed the difficulties and dangers of intubation, and pointed out that in his experience pushing down of the membrane had been a rare occurrence, and that blockage and ejection of the tube were difficulties which could be got over by an intelligent nurse. He advised the use of larger tubes than those recommended by O'Dwyer for children, and emphasised the fact that intubation was an operation which commended itself to patients, and said that his results had been quite as good as those of tracheotomy in laryngeal diphtheria.

Dr Buist read a paper on the means which should be adopted to prevent occurrence of ophthalmia neonatorum.

Dr M'Gillivray followed with a paper on the treatment of the disease when once established.

These papers will appear in a future number of the Journal.

Drs A. J. Duncan, J. Mackie Whyte, C. M'Vicar, and the chairman spoke on the subject. Dr M'Gillivray replied to the remarks on treatment.

Professor MacEwan gave notes of a case of subphrenic abscess on which he had operated and showed the specimen.

The patient was a man of 40 who had, five weeks before admission to the Infirmary, complained of abdominal pains after meals. A few weeks later the abdomen became suddenly swollen and remained so till admission. On admission he was very breathless and in great distress. The liver dulness was replaced by tympanicity entirely, and the hypodermic syringe withdrew nothing but air with fæcal odour.

Two incisions were made in the right hypochondrium and the abscess cavity evacuated and drained. The patient failed to recover from the operation.

The *post-mortem* showed a perforated ulcer of the first part of the duodenum which had become adherent to the under surface of the liver, whence infection had spread to the upper surface of the right lobe of the liver, forming a large subphrenic abscess.

New Books and New Editions

A Text-Book of Anatomy. Edited by FREDERICK HENRY GERRISH, M.D., Professor of Anatomy in the Medical School of Maine, Bowdoin College. Pp. 917, illustrated with 950 engravings in black and colours. London: Henry Kimpton. 1899.

UNDER the editorship of Professor Gerrish this excellent text-book on anatomy has been written by him and five other Professors of Anatomy in American colleges, of whom two are, like the Editor, also engaged in teaching and practising surgery. This makes three surgical anatomists out of the six authors, and ensures that due attention will be paid to those parts of anatomy which are important in practice. In the preface the authors say that they have aimed at limiting the descriptions to what is essential, while avoiding what is unimportant and exceptional, and they have carried out their intention effectively. Recent advances in anatomy are incorporated in the text; thus, for example, the abdominal viscera are described and figured from specimens hardened *in situ*, and the "anal canal" view of the so-called third part of the rectum is adopted without question.

In the description of the spinal nerves, however, there is no reference to the distribution according to roots. Illustrations of this subject, like those of Kocher, would be welcome in such a book as this.

Considerable emphasis is laid upon the study of external anatomy and the subject is illustrated with photographs and outline drawings, but we would venture to suggest that even more might be made of this department of the book. Something after the pattern of Holden's "landmarks" is wanted to

teach the student how to learn external anatomy, and to show him how interesting and valuable the subject is. It might be difficult to find the necessary room for such a chapter without striking out something else, but the paragraphs on microscopic anatomy of the tissues could easily be spared. They are not much wanted in a book of this kind.

The illustrations are mostly taken from a recent French work on anatomy (Testut), and are very clear and accurate. The use of red, blue, and yellow for blood vessels and nerves is a great help to the student. Some at first will be disappointed at not finding the exact places of origin and insertion of muscles marked out on the drawings of the bones. If they turn, however, to the description of the muscles, they will find there excellent original diagrams by Professor Gerrish, showing the exact origin and insertion of each muscle on an outline drawing of the bones. These, taken along with the drawings of the bones proper, will serve the purpose in some respects better than more complex drawings of each bone.

A new feature of this, as compared with other anatomical text-books, is a series of reproductions of X-ray photographs of living joints and parts of the trunk and pelvis. These are of service in showing the normal relation of bones to one another, and to the outline of the soft parts.

The book is admirably printed, and the arrangement of the paragraphs and judicious use of strong and italic type where suitable, makes the study of anatomy in these pages as easy and pleasant as may be.

We have much pleasure in recommending this as an admirable text-book for students, and as a useful work of reference for practitioners.

Intestinal Obstruction: Its varieties with their Pathology, Diagnosis and Treatment. By FREDERICK TREVES, F.R.C.S., etc. New and Revised Edition. Pp. 565 with 118 illustrations. London: Cassell & Co., Ltd. 1899. Price 21s.

MR TREVES' work on Intestinal Obstruction has for long been recognised as a standard authority on the various subjects with which it deals, and the new edition now before us will only

serve to confirm and extend its value as a work of reference. Since the first edition was published in 1884 the advances in abdominal surgery have been so great, and have taken place in so many directions, that the author has found it necessary in preparing the present edition to completely remodel it, and in many places to write it afresh.

After a brief introductory chapter in which the classification of the various causes of obstruction of the bowels is discussed, the subject matter of the work is considered under three headings: (1) the pathology and morbid anatomy of the different forms of the condition; (2) the clinical manifestations, and (3) the treatment.

It is in the section devoted to pathology, which extends to 284 pages, that we find most that is new in the present edition, and frequent references to these pages have already convinced us of the completeness of the author's studies of the different varieties of obstruction, and of the extreme value of the summary of the present-day position of the subject which his work contains. While nearly all the old and familiar illustrations have been retained, many new pictures have been added and greatly enhance the value of the work.

In the section describing the clinical manifestations, as might be expected, there is less that is new, although various theories regarding the explanation of stercoraceous vomiting which have raised considerable discussion are reviewed at some length. A valuable chapter on the differential diagnosis of abdominal lesions causing obstruction, and the errors liable to be made in forming an opinion, sums up the clinical portion of the work. In spite of the clearness of the writer's dicta and directions, we fear that at the bedside the differential diagnosis of many cases will remain in doubt until an exploratory incision reveals the state of matters present.

In the chapters on Treatment we find the writer is as strong as ever in his advocacy of the use of opium in cases of obstruction. We cannot but express our opinion that his teaching on this point is dangerous and pernicious in the extreme, and our belief that many more lives are lost than are saved by the administration of opium in these cases. Many practitioners do not yet realise, to use Mr Treves' own words, that "operation in these cases is too often regarded as

a last resource. It should be regarded as the *first* resource, as it certainly is the *only* resource." When they do we shall find that the treatment of intestinal obstruction yields eminently satisfactory results, and that just in proportion to the promptness with which the patient is given the benefit of surgical aid will the necessity for the use of opium disappear.

Practical Text-book of Midwifery for Nurses and Students. By ROBERT JARDINE, M.D. (Edin.), M.R.C.S. (Eng.), F.F.P. & S. (Glasg.), Physician to the Glasgow Maternity Hospital, Glasgow. Cr. 8vo, pp. 245, with 36 illustrations. Edinburgh: William F. Clay. 1899. Price 6s.

TO the readers of this journal the work of Dr Jardine is familiar, and they will not be surprised to find that he has written a text-book of midwifery for nurses and students, characterised by elaboration of practical details and by great attention to obstetric asepsis. The author has done his work well, although he has, like many other writers of similar books, set himself the difficult task of addressing both nurses with little or no knowledge of anatomy and physiology and medical students with presumably a thorough acquaintance of these subjects; as a consequence, practically an inevitable one, the manual contains more than most nurses will require or will be able to master.

With regard to the subject matter of the book, it runs pretty much along the familiar lines of anatomy and physiology of the generative organs, changes in and diagnosis of pregnancy, disorders in pregnancy, including abortion, labour, etc.; but a slight deviation from the beaten track is made in the chapter on antiseptics and asepsis, in which the author expresses his strong preference for the aseptic method in obstetric practice, and gives minute details as to the manner of carrying it out from the standpoint of the monthly nurse. No doubt some teachers of midwifery will be ready to join issue with Dr Jardine on this question; at the same time the insistence on the principles of surgical cleanliness can do no harm, but only good, even if it be found well-nigh impracticable to carry it through in the management of confinements outside the hospital. In this connection it has to be noted that the writer is com-

pelled to recommend a glass or metal catheter (not, we hope, the old female metal catheter) instead of a gum elastic one, for the reason that the last-named cannot be satisfactorily sterilized. Here and there throughout the book are signs of hasty proof revision; thus, "saggital," on page 22, ought not to have been allowed to pass, and "tip of the last lumbar vertebra" is scarcely a correct or accurate anatomical expression for spinous process (p. 15). Further, the hints given on page 96, to enable the midwife to distinguish between true and false labour pains, are rather scanty. These are defects, but they are capable of correction in the second edition of this work, for we doubt not that such will be called for. The illustrations are mostly from the works of Fothergill, Croom, and Playfair; two plates by Dr John Lindsay represent rupture of the uterus, and come from the author's work on "Hæmorrhage and Eclampsia." The work closes with a chapter on infant feeding, at the end of which occurs a sentence in which the good effect of the advice contained in it is somewhat diminished by the enigmatic fashion in which it is stated—"As regards sweets, the fewer children have the better for them."

Recent Literature

CRITICAL SUMMARIES AND ABSTRACTS

GENITO-URINARY SURGERY

By DAVID WALLACE, F.R.C.S.Ed.,

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THE best or proper procedure to adopt in cases of **hypertrophied prostate** which have advanced so far as to necessitate operative interference, is still, *sub judice*; castration, vasectomy, Bottini's method by galvano-cautery, and Harrison's operation, quite apart from partial or complete prostatectomy, have all their advocates. Statistics immediate and remote from the date of operation are rapidly accumulating, and should be before long available and suitable to enable us to estimate the relative results in regard to relief of symptoms, temporary or

permanent, and the mortality from the different operations. Von Frisch, at the end of a work entitled *Die Krankheiten der Prostata*, published a complete bibliography of the works upon diseases of the prostate during the last twenty-five years. This bibliography is deemed so valuable by Dr Delefosse, that he gives it *in extenso* in the October number of the *Annales des maladies des organes genito-urinaires*. Papers relating to prostatic affections can thus be readily looked up by those interested in the subject. The literature is so extensive that twenty-two pages are occupied by its enumeration alone.

In the treatment of bladder affections by cystotomy, the perineal and suprapubic routes are still adopted, and opinion differs in the minds of surgeons which method should be chosen in the treatment of certain conditions. A paper entitled "Comparison of the merits of Suprapubic and Perineal Cystotomy," by Professor Dandridge (*Annals of Surgery*, July 1899), and another by M. Poncet (*Acad. de Med.*, 1898) upon "Suprapubic Cystotomy and Cystotomy in Prostatic Affections," are of much interest. The views of Poncet are further brought before the profession in a treatise by M. Delore (*Paris, Masson*, 1899). Dandridge points out that both methods have dangers, but remarks that in each the condition of the kidneys dominates largely the mortality, and determines the degree of operative shock. This has an undoubted bearing in all surgical operations, and yet in many cases when kidney disease obtains operation is imperatively called for and the risk must be run. That the risk is even greater in the surgery of the urinary tract is recognised, but relief to urgent symptoms by operation may be a necessity, and one or other of the methods under review must be carried out. The real point at issue in these circumstances is, which is the safer, and to be chosen in the individual case? Is suprapubic cystotomy at the time a more severe strain on the patient than perineal cystotomy? To some extent the risk depends upon the length of time taken in the operation, but in this respect there is not much to choose, although probably as a rule the perineal operation can be more rapidly completed. On the other hand, however, in some cases the condition of the prostate necessitates a deep wound through a highly sensitive and vascular structure if drainage of the bladder is to be thorough, and the question of drainage is very important. Dandridge discusses the relative efficiency of drainage by the two methods in prostatic cases, and concludes that the perineal has advantages over the suprapubic. He believes the former is less dangerous, more rapid, and easier of execution than the latter; and further, that unless the suprapubic be supplemented by a perineal opening, drainage is not

so satisfactory. This seems to the writer only partially true, much depending on the condition of the bladder itself. If the bladder merely collapses, without contracting perhaps, although not certainly, the perineal drain is best; but if the bladder contracts after it is opened, as the normal bladder does, drainage is quite complete in suprapubic cystotomy. As regards comfort, in the sense of dryness, this is readily attained by the adaptation of the Sprengel pump, arranged as introduced by Cathcart. When the bladder merely collapses without contraction, by neither method is drainage so satisfactory, as urine tends to accumulate, and it is important to wash out the bladder from time to time.

Dandridge further refers to the tendency to the formation of a permanent fistula after a suprapubic cystotomy, but this results from the continuance of the prostatic obstruction, which at a later stage may be treated by prostatectomy or double castration as is thought best, and is required in the one method as much as in the other, unless a tube is permanently retained, which is equivalent to a permanent fistula. Here, however, there would seem an advantage in the perineal method, as the tube is almost at once so firmly grasped by the surrounding structures that leakage is prevented, while even when an attempt is made, on the plan of Poncet, to form a sphincter at the suprapubic opening, it is not always successful. Poncet reports upon 34 cases in which he performed suprapubic *cystotomy*. There was continence in 14 cases, partial continence in 7, and incontinence in 13. He is careful at the operation to stitch the different layers of the abdominal wall to the bladder, his object being to create an abnormal urethra, and thus combat the dangers of other modes of treatment in prostatic cases. The indications he gives are those usually accepted for one or other of the operative procedures referred to, but he advocates the adoption of cystotomy at the earliest period in preference to catheterisation, a view which has not yet found favour, and indeed is by those of the Necker school rather falling into disrepute as greater attention to purification of instruments, the surgeon's hands, and the patient's meatus and urethra (anterior) is carried out. Poncet divides cases for operation into two classes: (1) purely mechanical obstruction without sepsis, and (2) septic cases and those associated with kidney disease, and points out that the mortality depends on the general and local state of the patient, not upon the operation *per se*. In 117 cases of suprapubic cystotomy the mortality was 25·6 per cent., but of these in 42 in which the urine was aseptic there were 37 recoveries and 5 deaths, that is a mortality of 11·9 per cent.

Barling, quoted by Dandridge, in referring to cutting opera-

tions for stone, in six London and seven provincial hospitals from 1888-1892, found that out of 169 cases of the high operation there was a mortality of 15·3 per cent., while by the perineal route in 96 cases the mortality was only 5 per cent. Statistics are proverbially untrustworthy, but why this high mortality and difference in the two methods should be it is difficult to understand, unless there were complications which are difficult to estimate relatively in comparing two series of operations. Guided by statistics Dandridge would adopt the high operation for stone in a very restricted fashion, and would confine it to "stones of most unusual size, to encysted stone, and to stones complicating tumours, and to cases of some deformity, such as anchylosis of the hip—hypertrophy of the prostate should only occasionally demand it."

While admitting that suprapubic prostatectomy has advantages in the treatment of hypertrophied prostate, he believes that probably only partial operations should be attempted by that method alone, and that a perineal opening in addition to the suprapubic is advantageous, as hæmorrhage can be more readily controlled and the necessary drainage more readily secured. This second incision would no doubt materially assist in the operation of prostatectomy, but it seems to the writer unnecessary as he believes drainage can be satisfactorily got with the assistance of the Sprengel pump, and hæmorrhage, although it may be severe during operation, can be arrested by *hot* water, or, if need be, by the method of plugging described by Keyes and Cabot. In none of the cases of suprapubic prostatectomy, five in number, operated upon by the writer, has any great difficulty been met with in the arrest of hæmorrhage, and in all drainage has been quite satisfactory. Four of these cases made a good recovery and regained the power of micturition, and in none was there any tendency to permanence of the opening (vesical fistula). One case proved fatal, the least aggravated locally, the condition in it being due to a small, very mobile middle lobe about the size of a walnut, which was easily removed. The patient died on the nineteenth day after operation, the suprapubic wound being then closed. The kidneys at the autopsy showed old standing nephritis, but the bladder wound was cicatrised and the site of the third lobe not recognisable. Ample urine was excreted up to the day of death, and there was little albumen in it. The other organs were healthy.

Dandridge concludes his remarks upon the treatment of prostatic hypertrophy by saying that Harrison's operation—practically a perineal opening through the prostate into the lowest part of the bladder—may eventually prove the most

generally applicable, but the writer is of opinion that this conclusion is based on a mistaken view of the difficulty in, and insufficient drainage by the suprapubic opening. For the removal of vesical tumour the suprapubic route is said to be always indicated.

M. Forque, in the treatise upon surgery, edited by M.M. Duplay and Reclus (1899), advocates the suprapubic method of operation either as a means of temporary relief or for prostatectomy. Vol. vii. of this work on surgery is mainly devoted to genito-urinary surgery, is particularly well up to date, and the names of the authors—Tuffier, Forque, and Reclus—are a guarantee of its trustworthiness.

In regard to the rôle of suprapubic cystotomy as compared with the perineal route, it may be said that the former should be adopted when any further operative procedure is to be carried out within the bladder, after opening of the viscus, whether at the same time or at a later period. If removal of a tumour or portion of the prostate be proposed, or local treatment of a tubercular or simple ulcer, or if hypertrophied prostate so complicates the presence of a stone that lithotrity cannot be performed, suprapubic cystotomy is indicated. The perineal route may be adopted if nothing more than drainage be desired. Even, however, for drainage alone, there are cases where probably more relief will be given by the high operation than by the low, as, for instance, in bladders contracted in the later stages of tubercle, and in severe bleeding from inoperable vesical tumours.

The difficulty of the high operation has been exaggerated and the risks magnified, and it seems certain that the advantage of accurate determination of the condition present, only possible by a suprapubic cystotomy, counterbalances the rapidity of the perineal route in all but a small minority of cases. Immediate relief to symptoms is given, and if later a perineal drain is thought preferable or desirable, it can be carried out under more favourable circumstances by a second operation.

Röntgen rays in the detection of renal calculi.—In some patients the probability of the presence of a stone in the kidney is with more or less certainty diagnosed, but in others where the usual symptoms are indefinite or absent the diagnosis is extremely difficult, and arrived at chiefly by a process of exclusion. The simulation of renal calculus is occasionally presented in such dissimilar affections as appendicitis and movable kidney, and not infrequently nephrotomy is necessarily performed as an aid, in the first place, to diagnosis. It is only natural that in such circumstances every means at our disposal should be adopted to arrive at

accuracy in diagnosis, and thus when the Röntgen rays were introduced clinicians were hopeful that a ready means for the detection of calculi had been got. Repeated failure by the rays to detect stones which were afterwards ascertained to be present in the kidney have been reported, and that even in cases where, from the size of the calculi, the detection *a priori* should have been easy, and the conclusion has been forced upon those who have tried this method and failed that there has been some error in technique. Within the last year or two, however, various surgeons have successfully used the Röntgen rays in the detection of renal calculi. Albarran (*Ann. des mal. org. gen. urin.*, July 1899) gives a resumé of the work done in this direction, and draws attention to the chief points to be attended to in the application of the method. The first case was recorded by M'Intyre of Glasgow in July 1896. Albarran has collected from the literature on the subject twelve cases which have been fully reported, and six others which have been noted, and adds one under his own observation. Out of those cases the calculi in three were oxalate of lime, in five phosphatic, in two carbonate, and in one uric acid. All observers except Ringel agree that oxalate of lime gives the strongest shadow, and uric acid the least; while the shadow from phosphatic stones, though not so deep as that of the oxalate, is more so than that from the uric acid. Wagner (*Centrall. f. Chir.*, Leipsic, February 1899) lays much stress on the quality of the Crooke's tube and the length of exposure. Braatz corroborates the importance of the length of exposure, and Ringel points out that the size of the calculus, as we would expect, is an important factor in the shadow production. A positive result from radiography is most valuable, but frequent failure to detect calculi by its means, when later operation has proved their presence, shows that hitherto the rays have been unreliable, and that correct technique is of much importance. Whether small stones can be detected even now with certainty is questionable, but it is very desirable that the means most likely to be satisfactory should be carried out. Albarran, in the first place, points out two difficulties—(1) the thickness of the abdominal walls, and (2) the movement of the kidney during respiration. That movement of the kidney must be a barrier to successful shadow production, particularly of small stones, can be readily understood when it is considered that the stone may be moved through an area greater than its own diameter, as in such circumstances the shadow will be, if present at all, much less dense than if the stone were stationary. Regarding technique, Albarran says it is indispensable that (*a*) all the dorsal region

of the patient should be in as complete contact with the sensitive plate as possible. For this the legs and thighs should be fully flexed and maintained in position by a special apparatus; (*b*) it is necessary to protect the plate from external X-rays, and to prevent also all superfluous impressions due to diffusion of those rays in the air and to the halo which is always given by parts strongly affected; (*c*) the state of vacuum of Crooke's tube ought to be adapted to the purpose for which it is required, and maintained in that state during all the time of exposure. This state of the tube is that technically termed "a soft tube." (*d*) When these points are attended to, the length of exposure at a distance of 80 centimetres from the plate should be six to eight minutes.

Even, however, when these details are attended to, many patients will be difficult to satisfactorily examine by this method, and with the present difficulties in radiography as a means of diagnosis for renal calculi it would appear to be only of value in exceptional cases. If, however, by further improvements in technique small stones can be detected, its importance clinically will be great in difficult cases, and, for instance, in calculus anuria.

RESPIRATORY DISEASES

By G. LOVELL GULLAND, M.A., B.Sc., M.D., F.R.C.P.Ed.,
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So much has been written during the last year on the **open-air treatment of phthisis,** that I consider it quite unnecessary to say more about it. Readers who desire information will find it in plenty in the August number of *The Practitioner*, where there are papers by Dr Philip, the pioneer of the movement in Scotland, and others, and in Dr Walters' book on "Sanatoria for Consumptives." There is now no doubt whatever in the mind of the profession of the good results obtained, and the only point which is still necessarily unsettled is the degree of permanence which may be hoped for in the cure We want to know not only what the state of the patient is after five years, but after twenty years, and we want also a mass of *post-mortem* evidence to show us, when patients have died, let us hope of old age, from what extent of tubercular disease they recovered.

It is universally recognised that the earlier a case of phthisis is taken in hand, the more likely it is to be cured. But there is not yet a working agreement as to what is an early case. One constantly sees patients with extensive disease of both lungs who regard themselves, and who are regarded by their medical

attendants as early cases, because they are not greatly emaciated, because they do not have a constant evening pyrexia, and because the amount of cough and sputum is small. But one wants to begin to treat the disease at a much earlier stage, and any contribution to the **early diagnosis of phthisis** is therefore to be welcomed. Of course the discovery of tubercle bacilli in the sputum settles the matter, but it must be remembered that a negative result does not disprove the existence of phthisis. Repeated examinations must be made in a doubtful case, and even these may not always be decisive. I have met with two cases recently, in both of which the patients had suffered from undoubted phthisis for months, but where the sputum, examined by competent observers, did not show bacilli until within a few weeks of the death of the patients. Moreover, there are plenty of cases where at first there is little or no cough, and no sputum.

An interesting discussion on early diagnosis took place this year at the meeting of the American Climatological Association (*Bost. Med. and Surg. Journ.*), and Dr Williams, whose opinion was confirmed by several other speakers, declared that in several cases he had been able to show, by the aid of the X-rays and the fluorescent screen (not by radiographs), the presence of tubercular foci, which were quite inaccessible to auscultation or percussion. Dr Bonney insisted, very properly, on the examination of the *whole* naked chest, and on the fact that auscultation often gives earlier information than percussion. If no râles are to be heard, they may often be elicited by making the patient cough and then take a deep inspiration; but if they are quite absent at the time, attention should be directed to localised diminution in the intensity of breath sounds, to elevation of pitch or harshness, and to prolongation of expiration. These signs must, of course, be sought for especially at the apices posteriorly, and I may add that it is often possible to find apical shrinking before there is actual dulness present.

Bowditch (*Trans. Amer. Climat. Ass.*, 1898) lays stress on the combination of a weak and rapid pulse, more or less elevated temperature, and disordered or weak digestion. Other well-known points to which attention has again been recently drawn by Meissen, Loomis, Faisans and others, are the necessity for careful inquiry into the personal and family history for a possible source of infection, the occurrence of fatigue and dyspnœa on slight cause, irritability and depression in temperament, insufficient weight in proportion to height, and persistent loss of weight without apparent cause. The pulse is of great importance, rapidity without pyrexia, great variation in the number of beats on slight provocation, and, as Loomis insists, want of

influence by change of position on its rhythm, and again feeble arterial pressure, are all of importance. Murat (*Gaz. Hebdom. de Méd. et de Chir.*, 1899) calls attention to a subjective symptom shown by early cases where the focus is central. When these patients speak loudly they feel the affected side of the chest vibrating with the voice, and instinctively press the arm against the side to prevent this unpleasant feeling.

A doubtful case should be re-examined at intervals with the greatest minuteness, and as soon as the diagnosis is definitely made, the patient should be told with the utmost frankness what is wrong with him. It is cruel, if not criminal, to tell him that there is something the matter with one of his lungs which you hope will come all right in the summer, and yet it is done every day, and the patient's chance of recovery is often thus thrown away. The successful treatment of a case of phthisis depends not only on the minutest care and attention on the part of the physician, but also, to a greater extent than almost any other disease, on the intelligent co-operation of the patient and his friends, and this can only be given if the nature of the disease is understood.

As regards the **medicinal treatment of phthisis** we have long known of the good effect of arsenic, but there are some people who cannot take it in the ordinary forms. Renaut and Gautier (*Bull. de l'Acad. de Méd.*, 1899) find that the soda salt of cacodylic acid, an organic arsenical compound, which contains 54·3 per cent. of arsenic, is supported indefinitely even by rebellious stomachs in the dose of ten to twenty centigrammes in the day, and that nearly the same amount can be given hypodermically. They found it diminish fever, restore appetite, strength and digestive power, cause increase of weight, and promote cicatrisation of the lung. Guaiacol carbonate is another remedy which has done good service, but we have used it without a very clear understanding of its mode of action. Eschle (*Therap. Monatsh.*, 1899), on the ground of experiments by Freund and himself, points out that its real action is that of an intestinal disinfectant. It is broken up only by the putrefactive processes in the intestine and keeps these in check. The malnutrition of phthisical patients is brought about largely by the imperfect utilisation of their food: proteids, carbohydrates, and celluloses are alike destroyed by putrefaction which the insufficient pancreatic and intestinal secretions are powerless to prevent, and are thus lost to the patient. An exclusive meat diet does not supply the pabulum for the organisms which destroy carbohydrates and celluloses, and with the aid of guaiacol carbonate, or such substances as creasote, peppermint oil or ichthyol, brings about a return to normal digestion.

Of the substances which relieve cough, heroin is one of the most recent and most bepraised. Holtkamp (*Deutsch. Med. Woch.*, 1899) has found it useful in a large number of conditions such as acute laryngitis and bronchitis, influenza, dry pleurisy, phthisis, etc., where there was useless cough, and gave it either in doses of 0·005 grammes thrice a day, or in a single dose of 0·01 gramme at bedtime, in order to give the patient a good night. He found it useful also in whooping-cough in doses of 0·0005 to 0·002 grammes. Winternitz (*Ther. Monatsh.*, 1899), however, points out that the acetyl-derivatives of morphine, of which heroin is one, cause a distinct diminution of respiration and lessen the activity of the respiratory centre; that heroin in fact is a stronger morphine, and that it should not be used in young children, old people and weakly persons in such conditions as capillary bronchitis, where there is a distinct danger in diminishing the strength of respiration. Codein and dionin, which are alkyl-derivatives of morphine, have no influence on respiration in health and probably but little in disease.

May and Gebbard (*Deutsch. Arch. f. Klin. Med.*, Vol. 61) found in a case of **pneumo-thorax** that the gas contained in the pleura was inflammable, and consisted of CO_2, H_2, and N. This had been formed from the exudate by the action of bacterium coli, and they point out the interesting possibility that a pneumo-thorax might quite well occur without perforation of the pleura where this organism is the cause of the pleurisy, or is present in the effusion. Pleurisy due to the typhoid bacillus may produce a sero-fibrinous, hæmorrhagic or purulent effusion, with a high agglutinative power (Achard, *Sem. Méd.*, 1898), but does not seem to have any tendency to cause pneumo-thorax.

Diaphragmatic pleurisy is a condition which it is easier to suspect than to diagnose, but Zuelzer (*Münch. Med. Woch.*, Nov. 1898) has reported two cases, and Wild (*Corresp. bl. d. Schweiz. Aerst.*, 1899) a single case, which put our knowledge of it on rather a more definite footing. In Zuelzer's cases the pleurisy was purulent, in Wild's case there was no effusion. Besides the usual symptoms of pleurisy, such as pain in the side, cough, fever, etc., and diminished mobility of the affected side, there are two points which clinch the diagnosis in these cases. The first is tenderness, sometimes extreme, in the side of the neck along the course of the phrenic nerve on the affected side, the other is extreme sensitiveness to pressure at "de Mussy's point"—the intersection of the sternal or parasternal line with a line drawn horizontally forward from the end of the tenth rib. The coexistence of these two tender points is to be regarded as pathognomonic.

OBSTETRICS AND GYNECOLOGY

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The Implantation and Nutrition of the Ovum. Though it has no direct bearing on practice, the inquiry how the human ovum first takes its hold and is fed ranks as of the first importance. Leopold's article on the placenta in the last volume of the *Archiv. für Gynäkologie*[1] is of great interest to the obstetrician. It gives the result of the examination of ova from the commencement of the second and third months, and the third, fourth and fifth months, and also of the revisal of the specimens of the ovum at seven to eight days described in his Atlas.[2] Perhaps the most significant point in the paper is the admission of an error in his description of the latter. His description of this early ovum implied that the ovum became anchored on the free surface of the mucosa, and that the decidua grew up round it and enclosed it. In accordance with this view he introduced the terms "decidua basalis" and "capsularis" as equivalents for the "decidua serotina" and "reflexa" with which we are familiar. The evidence for this mode of implantation was the presence of a surface-epithelium, with gland openings on the serotina. As to the nourishment of the ovum at this early period, the space between the ovum and its capsule of decidua was taken up on the one hand by the proliferating villi, and on the other by maternal blood which had found its way there through the bursting of maternal vessels. Trabeculæ of decidual cells also crossed this space, breaking it up and foreshadowing the placenta. This view of the beginnings of placental circulation was, to say the least of it, very mechanical; it suggested a blood extravasation rather than a circulation. It was lucid but not convincing, the difficulty being not the blood finding its way out of the vessels into a space, but out of the space into the vessels again. A channel must be built before water finds its way along it, or the result is a marsh, not a stream.

Peters' description of an early ovum gave a more intelligible view of the commencement of both processes in the human female, and also brought them into line with the results of comparative embryology. In the specimen described by Peters, the appearances suggest that the ovum has forced its way into the mucosa and surrounded itself with a layer of trophoblast, in which the

[1] Leopold, Bott und Marchesi: Zur Entwicklung und dem Bau der menschlichen Placenta.—*Archiv. für Gynäkologie*, 1899, p. 516.
[2] Uterus und Kind, G. Leopold, 1897.

intervillous circulation has developed. The point of entrance has been closed by a coagulum. In commenting upon Leopold's specimen he draws attention to the fact that the upper pole of the reflexa consists not of decidual tissue but of fibrine, which may represent the coagulum closing the hole, and that the appearance of the decidua having grown up round the ovum may be due to the stretching of the decidual capsule by the eccentric growth of the ovum.

What he described as surface epithelium in his specimen, Leopold now considers "as nothing more than a strip of syncytium which has remained applied to the decidua after the villi have been torn away." He thus gives up the point that the ovum is resting on the free surface of the mucosa, or at least admits that Peters' description of how the ovum becomes imbedded in the mucosa may be the true one.

Whether the circulation begins at first in spaces arising within a shell of trophoblast is left undecided. That the spaces that become afterwards the sinuses of the placenta should appear in this way is a more intelligible explanation than the description given in Leopold's Atlas—that capillaries rupture into the space between the ovum and its decidual capsule. There seems no doubt that at a later period the proliferating villi break up the decidua and invade the maternal vessels. These vascular changes are fully described from later specimens in his valuable paper, which, however, throw no light upon the first relation of the fœtal and the maternal circulation. The changes in the decidual cells and in the glands of the serotina are minutely described.

It may be stated, therefore, as the result of the most recent inquiry, that the human ovum becomes imbedded in the uterine mucous membrane, not by a simple upward growth of the latter round the ovum so as to encapsule it, but by the ovum sinking below the free surface, the epithelium of which has either been previously shed or is destroyed by the growth of the ovum. This would make the reflexa "merely the superficial portion of the serotina," as Hart and Gulland have called it.

Disinfection of the Hands.—The disinfection of the hands is a subject which immediately concerns those engaged in midwifery practice. Krönig, the great authority on bacteriology in relation to obstetrics and gynecology, discusses this subject in a paper read to the Obstetrical Society at Leipsic.[1] He shows that, while for instruments and dressings the most rigid aseptic demands are made, the same cannot be said for the

[1] "Welche Anforderungen sollen wir an bakteriologischen Untersuchungen über Händedesinfektion stellen?" (*Centrall. f. Gynäk.*, November 11, 1899).

hands. We boil our instruments for ten minutes, expose dressings to sterilizing steam for half an hour, which would destroy even the spores of anthrax; but the hands cannot be thus treated. What is known as Fürbringer's method is the one generally adopted. In this the hands are scrubbed first with soap and water as hot as can be borne; then rubbed with an 80 per cent. solution of alcohol, and finally washed with a 1 in 500 sublimate solution. Considerable discussion has arisen as to the efficacy of this method, which will, of course, depend on the severity of the tests of asepsis applied. These Krönig discusses, and comes to the conclusion that the important point is not so much the strength of the antiseptic as a condition of the skin which will allow it to soak well in. He thinks the use of alcohol before the sublimate does not favour the penetration of the latter.

While recent work has shown that this method does not produce complete asepsis (micro-organisms persisting beneath the surface of the skin after the latter has been thoroughly cleansed), it is satisfactory for the obstetrician to know that in washing thoroughly with hot water, soap and turpentine, and then scrubbing with a corrosive sublimate solution, he obtains the nearest approach to asepsis possible in the case of the hands. The method advocated by Kelly of washing the hands with saturated solutions of permanganate of potash and oxalic acid is not of such practical utility for obstetric work, as these solutions cannot be so easily prepared.

DISEASES OF CHILDREN

By JOHN THOMSON, M.D., F.R.C.P.Ed.,
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Assisted by GEORGE ELDER, M.D., F.R.C.P.Ed., and
J. S. FOWLER, M.D., F.R.C.P.Ed.

Renal Lithiasis in Children.—Dr J. Comby, of Paris, read a paper on this subject before the Association of German Naturalists and Physicians, which met last September in Munich (*Arch. de Méd des Enf.*, Oct. 1899). His opportunities for gaining experience in cases of the kind had been curiously large, and his results are of considerable interest. Out of 600 *post-mortem* examinations which he had performed on young infants within two years, he had met with renal concretions no fewer than 100 times, although he had not found in that time a single case of gall-stones. Uric acid infarcts are very common in new-born children, and are seen even in still-born infants.

Dr Comby believes that the uratic sand which is derived from these infarcts may turn, in dyspeptic and atrophied infants, to gravel and actual calculi in the kidney, pelvis, ureter, or bladder. When calculi are present, they sometimes set up pyelitis or hydronephrosis.

Dr Comby gives short notes of 48 of his cases in which distinct gravel or actual calculi were present. From these we find that boys are more frequently affected than girls (31 to 17). As to age, 28 of the patients were under six months, 18 between six and twelve months, and 2 between one and two years. Forty-one of the children had been brought up on the bottle, in 29 there was a history of diarrhœa, in 24 of vomiting, in 28 of various skin eruptions. Athrepsia was present in 14 of the cases, and in 25 there had been rapid loss of weight. Most of the children had an atrophied, dried-up look, besides having broncho-pneumonia or some infectious disease, which contributed to favour the acidity and concentration of the urine and the deposit of urates, etc., in it.

Owing to the patient's age the *symptoms* are often obscure, if there are any at all. Renal colic is rarely recognised. Yellowish gravel is sometimes found on the diapers, and the child may have screaming attacks and be ill at ease, or there may be convulsions, dysuria, and retention of urine. Dr Emmett Holt and others have of recent years described, under the name of "Inanition Fever," a form of pyrexia which sets in in new-born children who are not getting enough milk from the breast, and which passes off when milk or water is given. This Dr Comby regards as merely a symptom of concentration of the urine and the consequent lithiasis. Generally, however, there is nothing pointing to the presence of concretions in the urinary passages.

The *prognosis* must be guarded. If the infant does not succumb to atrophy or some associated infection, his calculus may persist, and, becoming located in some part of the urinary passages, may increase in size until ultimately it causes some more or less serious symptom (renal colic, anuria, cystitis, vesical calculus, etc.). The lithiasis of adolescent and adult life often dates from early infancy.

The *prophylaxis* of the condition consists in proper nourishment from the beginning, either on the breast or by careful artificial feeding, along with the avoidance of too much food and of indigestible articles of diet, also of premature weaning. In febrile attacks it is important to give plenty of water to drink, in order to wash out the tissues and to encourage the function of the kidneys. When the stomach is intolerant, large enemata of boiling water may be tried, or injections of artificial serum. In confirmed cases of lithiasis, and in those in older children,

benefit may follow a course of treatment at such places as Contrexéville, Evian, Vittel, Capvern, and perhaps at Pougues or Vichy.

In the discussion which took place after the paper, von Ranke, Heubner, and Soltmann stated that they had never observed analogous facts. Although uric acid infarcts were very familiar, actual calculi had been, in their experience, but seldom demonstrated. Meinert asked whether the infants referred to as suffering from lithiasis had been accustomed to the use of alcohol, but Dr Comby stated that this was not the case.

Choreiform Movements after Chicken-pox.—Menko (of Amsterdam) records in the *Deutsche Medicinische Wochenschrift*, No. 45, a case of this kind. The patient, a girl of four years, showed the eruption of chicken-pox on May 23rd; the eruption was profuse and the fever pretty high. On June 4th she was noticed to be making grimaces, and the head and arms showed the presence of a tremor which rather increased with movement. There was some loss of balance when the eyes were closed. The pupils reacted normally; there was no nystagmus, and no movements of the external muscles of the eye. Speech was slow and difficult, but not scanning. She rapidly got well under treatment with bromide of soda and tepid baths.

This was, Menko thinks, a case of chorea minor. Disseminated sclerosis was excluded because of the absence of nystagmus and scanning speech, the normal sight reflex, and the rapid cure of the disease. The presence of the defective balancing power does not exclude chorea, as it is sometimes found in undoubted cases of that disease.

On Thoracic Resonance in Pneumonia and Pleurisy in Infancy.—Moussous (*Rev. mens. des maladies de l'enfance*, Jan. 1899), after describing the physical signs of pneumonia and pleurisy with effusion in infants, and pointing out how they differ from those of adults, states that in coin percussion (as practised in cases of pneumothorax) will be found a valuable aid in the diagnosis of effusion, and in mapping out its limits. In the healthy lung the sound is muffled and obscure and devoid of metallic character, and the same is heard in cases of broncho-pneumonia, tuberculosis, or other form of consolidation. When, however, there is an effusion, the sound of the coins is well conducted, sharp, and of metallic character. The stethoscope is applied where the physical signs are most evident, and gentle percussion is carried out with two coins at the opposite point of the affected side of the thorax. A table of cases in which this sign was evident is appended to the article.

Pleural Empyema in Children.—Vaux (*Montreal Med. Jour.*, 1899, p. 841) points out that three factors predominate in the etiology of empyema—pneumonia, septic processes in any part of the body, and tuberculosis. The first is the cause in two-thirds of all cases. There is strong evidence in favour of the view that when no bacilli can be detected by simple examination or by culture the case is tubercular. Vaux states that (in New York at any rate) Jewish infants are peculiarly liable to empyema. He regards the sense of great resistance noticed on percussion as only second in diagnostic value to the exploring needle. There are three indications for aspiration—(1) for the temporary relief of dyspnœa ; (2) to obtain pus for bacteriological examination in order to determine the most suitable operation ; (3) to establish a diagnosis. He emphasises the necessity of exploring immediately before operating, even if pus has already been found, since the first aspiration may have completely drained a small empyema. The presence of indol in the urine is a valuable aid in the diagnosis of empyema, since absorption of pus takes place from the pleura to a notable degree, thus differing from a mammary abscess, for example, whose pyogenic membrane prevents its occurrence to so marked an extent. As regards the choice of operation, Vaux advises resection of a rib (1) in tubercular cases ; (2) in long-standing cases with thickened pleuræ ; and (3) in some cases of streptococcal infection. Simple incision has the advantage of simplicity and rapidity, and probably leads to a more speedy recovery. It is indicated (1) in all cases where even the slightest shock must be avoided ; (2) when time is the first consideration ; and (3) in the empyema of healthy children, where only sero-purulent fluid is present.

OPHTHALMOLOGY

By ANGUS M'GILLIVRAY, M.D.,
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The So-called Embolism of the Central Artery of the Retina and its Branches (by Reimar, *Arch. f. Augenheilk*, vol. xxxviii., pp. 209). — Since the time of von Graefe's report, we know definitely the clinical picture of a case of embolism of the central artery, but, as it is well known, there are other affections which may simulate this clinical picture. According to Reimar, in such cases one must determine whether the blood current has been actually stopped. He believes that as long as the blood column appears to be unbroken, even though thinned from thickening of the

vessel wall or obscured by opacity of the vessel wall, and extends to the periphery without a break, circulation still exists; only when the column is broken up and the segments remain still, can one conclude that circulation is completely stopped. With the clinical picture of so-called embolism of the central artery there are three possible causes of the disturbance in circulation; embolism, thrombosis, and endarteritis proliferans. Reimar states further, that while the conception of thrombosis or of embolism will allow one to explain many of the symptoms observed, it will not allow one to explain them all; the conception of an endarteritis proliferans of the central artery or its branches, however, fills all the requirements, and this diagnosis is to be made when thickening of the arterial walls can be observed with the ophthalmoscope. In true embolism or thrombosis, which of course occur, we must have complete blindness and granular breaking up of the blood column beyond the obstruction.

The Ocular Evidences of Hysteria (by Casey A. Wood, *American Journal of Medical Sciences*, April 22, 1899).—Wood gives in this paper the histories of seven cases of ocular manifestation of hysteria. Some of the conclusions he draws from a study of these cases are the following:—Some symptoms, such as reversal of the ocular fields, the tonic form of blepharospasm, spasm of accommodation and convergence, and pseudo-paralytic ptosis, may be regarded as pathognomic of hysteria. Defects of vision are, generally speaking (with otherwise normal condition of the eye structure), hysterical, if accompanied by photophobia and any form of blepharospasm. Hysterical eye troubles are common in men and children.

Partial Substitution of the Cornea (by Kuhnt, *Ophthalmic Record*, October 1899).—An experience with nearly a thousand patients in the last fifteen years, with ulcerative processes, perforations after ulcerations or after the removal of fresh or old prolapse of the iris, fistulæ, keratoceles, incipient partial staphylomata, injuries with or without an opening into the interior of the eye, or after various operations, all treated by applying a flap of the bulbar conjunctiva, with or without a stem, as a substituting plastic material, enables Kuhnt to affirm the extreme benefits derived from this simple process. The shape of the eye is retained, and sometimes part of the vision, even after severe injuries or lesions. The flap is sometimes left attached at both ends, sometimes reinforced by a second flap, freshening the edges of the wound before covering it. In case of an ulceration, after evacuation and cleansing the flap is

applied, and the success is surprising. Pain disappears and healing is complete in five or seven days, after which the flap gradually vanishes, finally leaving nothing but a kind of pannus tenuis.

Parenchymatous Keratitis in Acquired Syphilis (by Roubiceck, *Ophthalmic Record*, October 1899).—While specific treatment is unmistakably effective in this affection with acquired syphilis, mercurial inunctions have no effect on congenital cases. Hence inunctions are not applied to children in Deyl's clinic. Roubiceck reports several observations which differ in several points from the scheme established by Valude; he claims that in the case of acquired syphilis the keratitis is unilateral, disproved by two of the observations; also that the pain is less than in the congenital cases, with no lachrymation or shrinking from light, which was the reverse of what was observed in another case. But the observations confirmed his other points: the lesser vascularisation with acquired syphilis, and the fact that the cloudiness of the cornea commences at the edge with distinct cloudy patches which slowly merge together. With congenital syphilis, the tongue-shaped patches of cloudiness soon spread over the entire area of the cornea. Many cases of the affection, with acquired syphilis, have probably been included with the congenital, especially in children, as we are apt to forget that children can acquire syphilis. Roubiceck considers it almost impossible to distinguish between interstitial keratitis from acquired or congenital syphilis, as experience has shown that Hutchinson's stigmata can occur with acquired syphilis. Fournier advises the following questions:—syphilis in the family; do the parents acknowledge it? syphilis stigmata on them; occurrence of abortions, premature births, eclampsia and convulsions, bearing in mind the possibility of lead and tobacco intoxication, as women in tobacco factories abort regularly.

Notes

PRIZE FOR RESEARCH IN TROPICAL DISEASES.—*University of Edinburgh, Faculty of Medicine.*—A number of medical practitioners in the Straits Settlements having presented to the University of Edinburgh a prize of the value of twenty-five guineas for Research in Tropical Diseases, the Faculty of Medicine has determined that the prize shall be open for competition to students of Medicine, or graduates in Medicine of not more than two years' standing; that competing essays shall be given in on or before the 30th of April 1900, and that any essay containing original, scientific, or literary research on Tropical Disease shall be eligible for competition. The follow-

ing are suggested as appropriate subjects of research, in order to aid candidates in their selection of a subject:—(*a*) Investigation on the conditions producing attenuation and exaltation in the activity of plague virus, and the relation of such changes to the symptoms of plague in man; (*b*) The bearing of meteorological conditions on the incidents of Tropical Disease.

ROYAL COLLEGE OF SURGEONS, EDINBURGH. — After having passed the requisite examinations the following gentlemen were, at a meeting held on 15th inst., admitted Fellows of the College:—Hugh Bennett, M.B., C.M.Edin., Captain, Indian Medical Service; Charles William Booker, M.R.C.S.Eng., Lee, Kent; John Clark, M.D.Edin., Cleator Moor, Cumberland; William Henry Cooke, M.R.C.S.Eng., Bath; Charles William Dean, L.R.C.S.E., Lancaster; William Evans, M.R.C.S.Eng., London Hospital; Alfred Joseph Martineau, M.R.C.S.Eng., Nottingham General Hospital; Alexander Glen Park, M.D. Glasg., Bolton-le-Moors; David Richard Rowlands, M.B., C.M. Glasg., Swansea Hospital; Robert Basil Stamford, M.R.C.S. Eng., Heathfield, Alfreton, Derbyshire; and James Whitton, L.R.C.S.E., Omara Hospital, New Zealand.

The prize of one hundred guineas for an original unpublished Essay on a subject in any branch of Surgery, or in Anatomy, Physiology, Therapeutics, or Pathology, bearing on Surgery, open to all Fellows and Licentiates of the College other than members of the President's Council, was awarded from amongst the competitors to Dr Arthur Logan Turner, F.R.C.S.E., 20 Coates Crescent, Edinburgh, the title of whose essay was "Racial Characteristics of the Frontal Sinuses."

The Gold Medal presented to the College by Colonel William Lorimer Bathgate, in memory of his late father, William M'Phune Bathgate, Esq., F.R.C.S.E., Lecturer on Materia Medica in the Extra-Academical School, was awarded after the usual written examination in Materia Medica to Mr David Mitchell, Student of Medicine, The Cottage, Broxburn.

Notes on Foods, Drugs, &c.

VIBRONA

Messrs Fletcher's Syrups of the Hydrobromates have long had a deservedly high reputation. Under the name of Vibrona they are now supplying a wine containing a fixed amount (2½ grs.) of the Hydrobromate of Cinchona to each fluid ounce. The taste is slightly bitter but by no means unpleasant, and the claim of the makers that the use of their preparation is not followed by the unpleasant symptoms of Cinchonism appears to be warranted by experience.

There are also supplied, Vibrona Sherry, Vibrona Champagne, and Vibrona and Malt, each of which has its own sphere of usefulness.

Analysis of the various preparations shows them to contain just what they claim to do, and in cases where tonics of this nature are indicated, Vibrona may be confidently recommended.

CADBURY'S COCOA

Messrs Cadbury have forwarded us samples of some of their preparations for report. The great value of Cocoa as a food and as a beverage is now universally recognised, and the very fact of this universal recognition has led to the sale of too many cocoas which are far from being above reproach. One need, however, be under no anxiety in regard to the cocoa of Messrs Cadbury; it is pure cocoa, all cocoa, and nothing but cocoa. Their breakfast cocoa is easily made, is pleasant to the taste, and easily digested, and the well-known value of cocoa as a food makes it an excellent start for the day in this cold weather.

They have also forwarded samples of their Mexican Chocolate, a very pleasant sweetmeat; but more desirable still, especially for children is their Milk Chocolate, which is not only readily taken by the children, but supplies them with an easily digested and agreeable food.

PLASTERS

From Messrs Beiersdorf & Co. we have received samples of some of their preparations, many of which must be familiar to many of our readers.

Their salicylic creasote plaster, associated with the name of Unna, still retains its place as easily the best of all the salicylic plasters, adhering well and being always active. Their mercury and carbolic acid plaster is another of which the merits are widely recognised, while the zinc oxide is an excellent and cleanly plaster of much general utility.

Their Tricoplasts, which were designed on the suggestion of Dr Arning (of Leprosy fame), are elegant and efficient substitutes for the more simple salicylic soap plasters. Though dearer in the first instance, the entire absence of any waste greatly reduces the disparity.

SOLOIDS

Messrs Burroughs, Wellcome & Co. have forwarded us a number of new tabloids, all of them likely to be useful in some direction. The most interesting are their new aniline stain tabloids for microscopic work. Anyone who has laboriously weighed out small quantities of methyl-blue or similar stains, and then spent half an hour in washing his hands, will appreciate the neat little one-grain tabloids which only require the addition of water to make up solutions of any desired strength. As stains they are equal to any on the market, and by making scientific work easier to the busy practitioner, whose time is valuable, deserve a warm welcome from the profession.

We have submitted their Fehling test soloids to a thorough examination, and find it necessary somewhat to criticise the directions for use which accompany them. If the two soloids are placed in a tube together and exposed to gentle heat, they seem to unite into a more or less insoluble compound. If, on the other hand, the alkaline tartrate is dissolved in boiling water and the cupric sulphate one added subsequently, a reliable test solution is obtained.

NOTICE.

In future all communications to the Editorial Department, including Exchanges and Books for Review, should be addressed to THE EDITOR, Dr W. RUSSELL, 3 *Walker Street, Edinburgh; and all business communications, including Subscriptions, Advertisements, &c., to* THE SUB-EDITOR, Dr NORMAN WALKER, 7 *Manor Place, Edinburgh.*

THE SCOTTISH
MEDICAL AND SURGICAL
JOURNAL

OPHTHALMOBLENORRHŒA (PURULENT OPHTHALMIA) NEONATORUM[1]

I. From the Obstetrical Standpoint

By R. C. BUIST, M.D.,
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OF the importance of this subject it may be enough to say that a gathering of superintendents of asylums for the blind, in 1876, estimated that on the average one-third of the blindness was due to this cause. The immense improvement that has taken place in the result of treatment since that time makes it certain that this proportion must now have been greatly reduced; but the affection is yet common enough for all of us to have seen some cases, and probably a few serious enough in their intractability and in their result. Up to the seventies the disease was of imposing importance from the mere frequency of its occurrence, 10 per cent. of the children born being almost the minimum rate even for large and carefully conducted hospitals such as those of Leipzig and Dresden and Edinburgh. At that time much improvement was taking place from the better knowledge of cleanliness, and since the early eighties a purulent ophthalmia rate amounting to 1 per cent. of the total births is excessive, and in many places a case is now scarcely ever seen.

For clearness sake it may be well to state here that all cases of purulent ophthalmia are not specific, the pneumococcus and other organisms having been identified as causal, though less commonly than the more virulent gonococcus. The proportion of simple catarrhal cases seems to be about one-third.

[1] Read before the Forfarshire Medical Association.

but inasmuch as the practitioner must practically treat every case as if it were specific, it will be unnecessary to separate them further in our discussion. In these cases it is by no means clear what the source of infection is, or that it is in the perhaps leucorrheal discharges of the mother. The secretion of nongonorrhœal ophthalmia has failed to cause experimental ophthalmia in animals, and is possibly not infectious, and there is no definite relation between ophthalmia and puerperal morbidity in an establishment.

It is needless to elaborate the history of the attempts made to avoid this affection, but a short note may be both interesting and useful in illustrating the successive stages in the progress. Abegg of Danzig, by merely washing with pure water immediately after birth the eyes of all children, reduced the disease rate to 3 per cent. in the years from 1871-80. Bischoff of Basle, by simple cleanliness, reduced his rate to 3·5 per cent. in 1873-4, and further, in 1874-5, to 2·6 per cent., by using carbolic vaginal injections and washing the eyes with salicylic acid. The classical paper on this question was published by Credé of Leipzig in 1881. He had first tried frequent douching of the vagina in all the women admitted, and in the years from 1874 to 1879 his disease rates were 13·6, 12·9, 9·1, 8·3, 9·8, 9·2 per cent. of all the births. In the first half of 1880 he adopted washing the eyes with solutions of borax or silver nitrate, and the rate fell to 7·6 per cent. In the second half of the same year he instilled a 2 per cent. solution of silver nitrate into the eyes of all children, and the rate fell to ·5 per cent. There have been various modifications in detail in the carrying out of the method, but its essential feature has always remained the instillation of the 2 per cent. silver nitrate solution. Credé's results were soon confirmed by other observers, including Fehling of Stuttgart, Braun of Vienna, and A. R. Simpson, and to the present his method is routine in many hospitals. In 1896, Köstlin made a comprehensive report which showed, in 24,724 cases treated by Credé's method, a disease rate of ·655. Corrosive sublimate in 460 cases gave ·43 per cent., but is more dangerous than silver nitrate.

With results so good as these it would have been unnecessary to make any change in our methods, were it not that on the one hand so strong a solution as 2 per cent. silver nitrate

causes sometimes irritation of the conjunctiva, and on the other the great change in our processes of cleansing has made many measures needless that were before essential. It is, therefore, well to review from time to time the various points in a problem such as this of the prevention and treatment of ophthalmia neonatorum, and to study the application of any additional knowledge that the years may have brought.

There are well-authenticated cases of the presence of an actual ophthalmia at the time of birth, and it is known that in cases of premature rupture of the membranes, ophthalmia is more common. It bears, however, no relation to the duration of the period of expulsion, and there is evidence that the eyes rarely become infected during the passage of the head through the birth canal. From the moment of birth of the head the danger is continually present. The eyes are now opened and exposed to the agents of injury. They are, moreover, from deficiency of the tear secretion and the relative lack of movement, less able to deal with any noxious agent than they later become. The hand of nurse or doctor may introduce virulent discharge from the mother's vulva. The sponge or cloth used to wash the face may be not only surgically unclean but filthy. The daily ablution, the rubbing of the face on the mother's breast, or the caress of her hand that has been in contact with uncleansed parts of her own person, may cause an affection secondary to any possible disinfection of the eyes at birth. Shall we then employ a routine prophylaxis, and, if so, what method shall we adopt?

The answers of the authorities vary somewhat.

1. The London Obstetrical Society, in its "Instructions to Midwives" who hold its certificate, says: "As soon as the child's head is born, and, if possible, before the lids are opened, its eyelids should be carefully wiped with pledgets of absorbent wool soaked in corrosive sublimate solution (1-4000), and, as soon as practicable after birth, a few drops of the above solution should be dropped into each eye."

2. Dakin says: "The eyes, if not attended to earlier (before the mother is attended to), should be very carefully washed with clean warm water to guard against ophthalmia; and if there is a suspicion of any purulent discharge in the mother, a solution of perchloride of mercury (1-2000) should be dropped into their

outer canthi." Credé, who was the first to advocate and to prove the clue of this practice, used a 2 per cent. solution of nitrate of silver in every case.

3. In Edinburgh, Haultain and Ferguson say to obstetric nurses: "The greatest caution ought to be taken in washing the infant's eyes at the time of birth with some lukewarm corrosive sublimate solution (1-2000)."

4. Fothergill says that in hospital and dispensary practice it is usual to drop into the eyes a little (1-2000) corrosive sublimate solution.

5. In Dublin, in 1886, Lane said that treatment is not adopted till the disease is actually manifest, and then both eyes are treated with silver nitrate solution (8 grains per ounce).

6. Jellett says that prophylactic treatment should be adopted as routine in hospitals, and also occasionally in private practice, wherever there is reason to suspect gonorrheal infection in the mother. It consists in wiping the eyes of the child carefully with a soft rag the moment the head is born, and then in dropping in two drops of a solution of nitrate of silver of a strength of four grains of the nitrate to an ounce of water.

7. The "American Text-book of Obstetrics" says: "Immediately after birth, before the child has opened its eyes, all secretions on or about the lids should carefully be washed away with sterilized water or with a 1 or 2 per cent. solution of boric acid. In many hospitals Credé's method is that usually adopted."

8. Hirst says that Credé's method is the best preventive treatment.

9. In Vienna, Schauta says: "In the interval between the completion of the birth and the cutting of the cord we disinfect the eyes and the mouth of the child. The eyes are washed externally with boiled water and clean wadding by wiping the lids repeatedly from within outwards. After cutting the cord it is well to repeat the cleansing, and to drop into the half-opened lids a few drops of 2 per cent. solution of silver nitrate.

10. Ahlfeld, whose text-book contains an unusually elaborate discussion of this question, says that prophylaxis consists in repeated cleansing of the vulva shortly before the exit of the

face, and in the immediate washing of the face after the birth of the head, and he describes the manner of performance. In cases where the mother has an evident mucopurulent discharge, Credé's plan of instilling a 2 per cent. solution of silver nitrate is adopted. Ahlfeld adds that it has still to be determined whether the undoubtedly favourable results of this practice are due to the nitrate or to the universal practices of cleanliness in maternity institutions. His own opinion apparently leans towards the explanation by cleanliness, for he points out the superficial and transitory action of the nitrate. He also indicates that the nitrate has disadvantages in the reaction it causes, and that this may itself simulate incipient blennorrhea, or for a time mask a true specific affection, of which, as he points out, the method is not an absolute preventative. He further quotes a case from his wards where clouding of the cornea occurred in a case in which no gonococci could be found.

11. Abadie takes a very similar position. He refers to cases in which silver nitrate or corrosive sublimate has caused cloudiness of the cornea, which slowly subsided when the lotions were stopped. He also thinks that cleanliness is the best prophylactic. This consists in thorough disinfection of the mother, and in the child, the careful cleansing of the parts adjacent to the eyes, and particularly the skin of the lids, with boric acid solution or boiled water. Silver nitrate drops should only be used when there is reason to suspect gonorrhea of the mother, and then two days' interval should be allowed before repetition, to determine if the reaction to the instillation will disappear.

12. Lundsgaard also, whose work is of the most careful kind, says that many ophthalmias are mere conjunctivitis from irritation with silver nitrate, and that routine application of Credé's method is a mistake.

With the information before us, we may now summarise the discussion in the form of rules for practice concerning various details in the management of pregnancy and labour. If there is any opportunity during pregnancy ascertain whether the patient has merely the ordinary lubricating vaginal discharge or a purulent one. If purulent, examine for evidence of gonorrhea, and undertake treatment without delay. If it be only after the onset of labour that an abnormal discharge is discovered, a

simple detergent vaginal douche may be given, antiseptics seeming to exert no favourable influence. *The essential measure* at this period is the scrubbing of the vulva, and this should never be omitted, but should be repeated if possible. If the membranes have been preserved till just before delivery antenatal infection is not likely, but if they have ruptured early the converse almost is true. When the head is born wipe the eyes with clean absorbent cotton. Take a fresh pledget for each stroke. If you prefer to use lotion, physiological salt solution or boric lotion are probably best. Corrosive sublimate solution (1-2000) certainly produces irritation, and if your selection falls on this drug you should use it in a much weaker solution. In washing the child, the order of procedure should be eyes, face, scalp, and no swab or cloth that has touched the scalp should be allowed to come near the eyes. If you think the mother has gonorrhea it is well worth while to use Credé's method. After the child has been washed, again cleanse the skin about the eyes, and then separating the lids with finger and thumb, instil from a nippled dropper two drops of silver nitrate solution (2 per cent.) 10 grs. per ounce of water. During the puerperium look at the child's eyes at each visit, including the last, and if you find the least purulent discharge or other sign of possible infection order the instillation of silver nitrate solution into each eye. I use only one grain to the ounce, and order its insertion every hour, and whether from the absence of infective cases or the influence of the drug have had little trouble. If one eye only is affected have the child kept on the affected side. If in spite of this there is no sign of diminution of the discharge, or if it increase, consider the case one of specific infection, and hand the treatment over to the ophthalmologist, with whom the further treatment of the theme must also lie.

II. From the Ophthalmological Standpoint

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OPHTHALMIA neonatorum may be said to be identical with purulent conjunctivitis, *i.e.* blenorrhœa adultorum, for both are produced by the same cause, viz., infection from genitals

affected with virulent catarrh. Clinically, however, blenorrhœa neonatorum differs from that of the adult, in that the symptoms are less severe, and the chemosis of the bulbar conjunctiva is relatively slight, so that corneal complications are less frequently met with, and the disease is altogether more amenable to treatment. Nevertheless, once ophthalmia neonatorum is established, we are face to face with a most dangerous condition. It is then essential that the utmost attention and care be paid to the case, and that the treatment be begun as early as possible, for by prompt, careful, and energetic treatment a perfect and complete recovery may invariably be expected. The obstetrician should systematically examine the child's eyes at each visit during the ordinary period of his attendance, for by so doing he will be able to recognise the disease at an early stage; and by at once bringing it under therapeutic influence, deprive it, to some extent at least, of its virulence.

The disease, when neglected or left to run its course, is extremely fatal so far as vision is concerned, as it invariably attacks both eyes. According to Fuchs and Magnus one quarter of all the blindness in the civilised world is due to blenorrhœa neonatorum, and in Germany and Austria more than one third of the inmates of Blind Asylums were rendered blind by this disease. In the United States of America at last census, Swan M. Burnet found that of upwards of 50,000 totally blind people more than 25 per cent. lost their eyesight through neglected blenorrhœa neonatorum. In 1884 Sir William Bowman stated that in Great Britain some 7000 persons were rendered blind from ophthalmia neonatorum, and to these an equal number might be added to represent those suffering from the effects of the malady in less severe forms.

If the disease be seen to be established the eyelids should be carefully everted, and their conjunctival surface brushed with 2 per cent. nitrate of silver solution, after removing any discharge with tepid water and mops. As with the child there are few tears, and but little sodium chloride in what tears there are, there is no need to mop up the tears before applying the silver nitrate solution as is done in the case of the adult, where the sodium chloride precipitates the nitrate into the insoluble and inert chloride of silver.

The rule throughout the treatment should be, never to

touch the eye if possible, lest the cornea be scratched or injured, for, once the cornea is abraded, the risk of its destruction is tenfold.

The irritant action of the silver nitrate may to some extent be neutralised by bathing the eyes, a few seconds after its application, with a weak salt solution, which will precipitate any silver nitrate left on the surface. After the brushing, the bulbar conjunctiva loses its fleshy red colour, through precipitation of the albumin contained in the superficial epithelial cells. These cells, thus rendered opaque and dead, soon become separated from the living ones beneath, and with them the micro-organisms contained in these layers are removed, dead or alive, from the scene of action.

The application of the silver solution should be repeated daily (seldom twice a day except in very severe cases), and continued till the discharge has ceased, when a mild astringent lotion such as alum and boracic lotion (grs. 2 and 10 respectively to the ounce of water) should be substituted till complete recovery is attained.

Protargol, a recent remedy, which is highly praised by Darier of Paris and others in almost all conjunctival inflammations as an efficient substitute for nitrate of silver, is still on its trial. It is said to possess all the advantages without the disadvantages of the older salt, but from recent writings this appears to be doubtful. Meanwhile I am conservative enough to depend on nitrate of silver in spite of its disadvantages, trusting it as an old and tried friend.

Almost as important as the application of the silver nitrate solution is the frequent and careful removal of all discharge by gently separating the eyelids with finger and thumb, and douching the eye with some mild or neutral solution, preferably tepid physiological salt solution—0·6 per cent.

When physiological salt solution is taken, it may be employed every half hour if the purulent secretion be copious, and, if used at body temperature, may be applied during sleep without waking the child. Its use for this thorough and frequent removal of secretion from the eyes cannot be too firmly impressed on the nurse. In the case of a strange nurse the doctor should himself show her how to remove the discharge, never taking it for granted she is qualified to do so.

If mops of cotton wool charged with fluid be used for douching purposes, they should not be allowed to come in contact with the eye; while, if any of the many patterns of douches sold by instrument makers be used, the nozzle, even when tipped with indiarubber-tubing, should not be allowed to touch the eye. Only by adhering to such rigid rules can the cornea be secured against abrasion. The nurse in douching must always be careful not to allow the fluid spurt back into her own eyes (especially when douches are used), and she must also be careful that proper precautions be instituted against disseminating the infection among the family. So much depends on the intelligence and conscientiousness of the nurse or attendant, that a little time spent in instruction is well repaid. Besides the douching just alluded to, the employment of a mild antiseptic lotion to the eyes, such as perchloride of mercury 1 in 5000, boric acid 1 in 50, or formol. 1 in 2000 every four hours, appears to be helpful. When the discharge is profuse, and especially when gonococci are found, I bathe the eyes with formol. solution every two hours, after removing any secretion with the salt solution. The edges of the eyelids should be smeared with some mild antiseptic ointment or pure vaseline, to keep the eyelids from becoming gummed together, and so preventing the escape of secretion through the palpebral apertures.

Should corneal complications set in, nitrate of silver is not contra-indicated. It should still be continued daily, together with frequent douching with physiological salt solution, the application of formol. solution (1 in 2000) every two hours, and atropine drops (grs. 4 to the ounce) thrice daily. Once ulceration has set in, pressure even of a very slight nature on the eyeball may cause perforation of the cornea. This danger must be carefully pointed out to the nurse, for, when perforation takes place, the vision of the eye is invariably lost through more or less complete adherent leucoma, and occlusion of the pupil, etc. In extensive perforation the lens and vitreous may escape and shrinking of the eyeball follow. Under energetic treatment, however, on the lines indicated, ulceration of the cornea may often be combated and perforation prevented, notwithstanding the virulence of the attacking force, for the corneal tissues of the infant possess highly resisting and recuperative

properties, and it is often astonishing how little cicatricial tissue (leucoma), as compared with the adult cornea, is to be found in those cases where ulceration has been combated.

Nystagmus is a common complication found after blenorrhœa neonatorum, and, once set in, it is incurable. The vision of the eyes becomes considerably impaired, so that the patient, although he may have made a perfect recovery otherwise, is thus handicapped for life. The nystagmus is supposed to be due to the closure of the eyelids during the early part of life, when co-ordination of the extrinsic muscles is acquired. Every attempt should therefore be made to reduce the swelling of the eyelids, that the child may open the eyes as early as possible.

With regard to the strength of the silver solution, namely 2 per cent., some use it weaker, and a few stronger, the advantages of the 2 per cent. solution are, first, that one can regulate the effect more readily by making a slight or a prolonged application with the brush, and second, when used in this strength, it is less painful than when weaker. It is well known that nitrate of silver applied lightly, and especially in weak solution, irritates the nerve endings in the conjunctiva, while used freely and in 2 per cent. solution, these nerve endings are benumbed, and consequently there is less pain. I only use the weaker solutions, such as 1 per cent., when a membrane—not diptheritic—is found covering the palpebral conjunctiva. Such membranes are easily separated from the inflamed parts below, and they are not to be regarded generally as a serious complication.

Argyrosis seldom if ever happens in the treatment of blenorrhœa neonatorum, because the applications of the silver nitrate are not required sufficiently long to produce this discolouration of the conjunctiva.

Constitutional treatment should be considered, but, as a rule, it is seldom specially required. Fresh air should be enjoined when possible, and the popular idea of further catching cold in the eyes must be uprooted.

Should one eye alone be affected, the child should be laid in its cradle so that the side of the face, corresponding to the affected eye, be lowermost; and care should be taken not to touch the good eye with anything which has been in contact with the bad one.

The disease under treatment usually runs a course of from three to six weeks. Improvement is observed when the discharge becomes less, and the swelling of the eyelids subsides, as evidenced by the child opening his eyes and the wrinkling of the skin of the upper eyelid.

Seeing then that such cases are to a very large extent preventable, or, when established, curable, have we in this country taken sufficient precautions against the ravages of the disease? From what has been said you will see that the matter concerns not only the family but also the state. Most European Governments, with the exception of our own, require compulsory notification of all cases of blenorrhœa neonatorum. In the United States of America, twelve states have already passed laws concerning compulsory notification, and the American Medical Association, on the recommendation of its Ophthalmological section, has sanctioned the approval of Credé's method in all public institutions. What has been done in Great Britain in the way of legislation? The Ophthalmological Society of the United Kingdom in March 1884 appointed an influential committee to investigate, as far as possible, the relative frequency of blindness from ophthalmia neonatorum in this country, and in June of the same year the following resolutions drawn up by the committee were adopted:

1. That the purulent ophthalmia of new-born infants being the cause of a vast amount of blindness, mainly because of the ignorance of the public regarding its dangerous character and the consequent neglect to apply for timely medical aid, it is desirable to instruct those in charge of new-born children by a card, in substance as follows:

Instructions regarding new-born infants.—If the child's eyelids become red and swollen, or begin to run with matter, within a few days after birth, it is to be taken without a day's delay to a doctor. The disease is very dangerous, and if not at once treated may destroy the sight of both eyes.

This to be distributed through the medium of the Poor-law and Birth Registration organisations of the united kingdom. In England the Relieving Officer, and in Scotland the Inspector of Poor should, in every case of labour under the Poor-law system, read to and leave with the person obtaining the order for medical aid, or the persons in charge of the patient, a copy

of the card. In Ireland the card should be attached to the order for medical aid in such cases, and the person who gives the order and card should, before doing so, read the card to the applicant. The Registrar of Births should read and hand to each person registering a birth a copy of the card.

2. That the advocacy and aid of the medical press be solicited in drawing general attention, and especially that of the authors of text-books on midwifery, of the lecturers on the same subject for students and midwives, and of the various institutions which train, and charitable institutions which employ midwives, to this important subject.

3. That a copy of the first resolution be forwarded to the respective presidents of the Local Government Boards of England and Ireland, and of the Board of Supervision in Scotland, and such other persons, if any, as may be necessary, and that a deputation be appointed to wait upon the said presidents and other persons, if necessary, and urge upon them the official adoption of the views therein expressed, and to take such other steps as they consider necessary. In volume V. of the "Transactions of the Ophthalmological Society," page 31, we find a detailed account of the communication with the Government on the prevention of blindness from ophthalmia neonatorum anent the above resolutions. Briefly stated the Local Government Board of Ireland took the matter up heartily, and addressed copies of circulars embodying the resolutions of the Ophthalmological Society to each Board of Guardians, to the medical officer of each district, and to each midwife. In England, however, even after a weighty deputation, including Sir William Bowman and Mr Jonathan Hutchinson the then president of the society, had waited on the representatives of the Local Government Board at Whitehall, no action was taken, and there the matter has rested. In Scotland likewise no action has been taken.

Besides general legislation a great deal can be done in communities in informing the laity as to the dangerous nature of the disease, and we in Dundee may note with pride that our energetic M. O. H.—Dr Templeman—has taken action in the matter in the circular which is distributed by the registrars at the registration of each birth. In this circular parents are told: "In washing the newly-born child, the eyes should first of all be

carefully cleansed with warm water. A clean rag or piece of flannel should be used for this purpose. Sponges should never be used. If after a day or two the eyes appear red or begin to matter, a doctor should at once be consulted. Neglect of early treatment may lead to permanent blindness. The discharge from the eyes is infectious."

Perhaps more important than the instruction of the parents would be that of the midwife. My experience of this class of attendant here is that there exists the greatest ignorance on their part as to their duties and responsibilities when brought face to face with this disease. The unfortunate child is wrongly treated, and in many instances the midwife displays the utmost reluctance in allowing the little victim to be taken to a doctor, or to the Eye Institution. It does not seem to me to be impossible to get at these midwives and instruct them as to the dangerous nature of the condition they apparently make light of. Our association might with credit attempt their conversion, and any small amount spent in disseminating knowledge on this subject among the laity would, I venture to say, be money well spent.

ON THE IMPORTANCE OF EXAMINING THE BLOOD IN DOUBTFUL CASES OF DISEASE[1]

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I WISH at the outset to premise that I do not intend to speak specially about the examination of the blood in such diseases as pernicious anæmia or chlorosis, for in these and similar conditions everyone is agreed that the blood *ought* to be examined, and such examinations are not infrequently actually made, even though the results are not always scientifically accurate. But there is, in this country in particular, a general and widespread ignorance of the amount of information which can be gained from the thorough examination of the blood in general diseases, apart from the diseases of the blood-forming organs, and of the valuable indications for diagnosis, prognosis, and treatment which

[1] Read before the Medico-Chirurgical Society of Edinburgh.

can be obtained in this way. I say in this country advisedly, for the vast bulk of hæmatological knowledge is derived from observations made either in the German-speaking countries or in the United States. One reason for this may be that we have not here such diseases as malaria to draw our attention to the blood in obscure conditions. There must, however, in addition be some fault in our education, for certainly not one in a hundred of our young graduates knows how to conduct a blood examination properly, or, which is even more important, how to interpret his results when he has got them; and if he has the knowledge, it is too often forgotten or neglected when he goes into practice. If students were less vexed with the skeleton of the crocodile, and with the phenomena of electrotonus, there might be time for them in their course to learn matters of value to them, and there would be some chance of medicine becoming a scientific profession instead of a business.

There is in the minds of some men a positive dread of making blood examinations. For example, I came across an instance not long ago of a practitioner who had to examine a case for life insurance. The proposer had an enlarged spleen, and the examiner suspected that he might have leucocythæmia. Instead of pricking the finger and making sure, as he might have done in ten minutes, the doctor gravely wrote on the form that "of course he had not examined the blood, as there was a considerable danger of erysipelas from the necessary puncture, and he feared to involve the company and himself in a lawsuit for damages if this happened!" In all the hundreds of punctures that I have made, and the thousands I have read of, I never heard of the slightest ill effect from the necessary needle-prick. A somewhat similar cautiousness on the part of a colleague once put me in some embarrassment. There was a question in a case whether the symptoms were due to pernicious anæmia or to a hidden gastric cancer. The case had been seen by a consultant very much senior to myself, who not only had made no blood examination, but had so impressed on the patient the danger of his losing a single drop of blood, that I had great difficulty in persuading him to let me examine it. The case, when examined, turned out to be a pernicious anæmia of the chronic variety, and did well on appropriate medication, which was determined by the blood examination. On the other side,

there is the man who expects you to diagnose pernicious anæmia without examining the patient and without counting his corpuscles, by the aid of two dirty coverglasses which have had a drop of blood squeezed between them, and which he has not always taken the pains to draw apart!

Perhaps the best way to press the importance of the matter is to point out that blood is the only *tissue* of the body which we have the opportunity of examining microscopically and chemically during life, and that in most other cases we can only examine tissue products. What would we not give to be able to examine a portion of the kidney in a doubtful case of albuminuria, for instance? Further, while there are special diseases which affect mainly the blood, there is probably no disease which does not affect the blood-mixture, as the Germans call it, in some way. Our present methods are often too rough to enable us to seize the exact difference; in most cases we can only trace comparatively coarse morphological changes, but there is no reason to believe that the discovery of serum diagnosis in typhoid fever and other infectious diseases has exhausted the possibilities in that direction. We may hope to find subtle changes in the diseases of metabolism, for instance, and there are many avenues of work whose openings we can see, but not their ends.

Leaving out of sight, however, for the time all these finer points, in an ordinary blood examination we have to look to the number and condition of the red corpuscles and their content of hæmoglobin; the number of the white corpuscles, and the percentage of the different varieties present; the number of blood plates, and the rapidity of formation of the fibrin network. In special cases, of course, such things as the plasmodium malariæ or the presence of organisms may have to be looked for, but these I need not now further allude to.

First as to the number and condition of the red corpuscles. These points are of importance in the diagnosis from primary blood diseases, but apart from that they are of comparatively little service in making the diagnosis of the disease which is present, except in so far as they enable us to discriminate between a grave illness and a slight one; for the reason that the conditions which produce anæmia are almost infinite in

number, and any two causes of equal severity will produce just the same kind of anæmia, no matter how widely diverse they may be. For example, gastric cancer, bleeding piles, and septicæmia will produce anæmias which, as far as the red cells go, are indistinguishable from one another. The condition of the red cells is invaluable on the other hand in helping us to estimate the severity of the individual case and its prognosis, the amount of recuperative power, and, of course, to trace progress. It is obvious that a patient suffering from, say pneumonia or phthisis, has a much better chance if he starts with 5,000,000 corpuscles to the cubic millimetre than if he has only 2,500,000, and those small and misshapen.

The percentage of hæmoglobin gives very much the same kind of information as the red cells; in many cases the amount of loss is proportional to the loss of red cells, and in many others—where the corpuscles are small and of light weight—the hæmoglobin is proportionally diminished. In one case only, that of pernicious anæmia, the hæmoglobin is relatively increased.

The number of blood plates is difficult to estimate, and changes do not usually give us any information that cannot be got in other ways. The same may be said with regard to the rapidity of fibrin formation; the one point in which we may get special help from it, is in making a diagnosis between suppuration and malignant disease. In the former the fibrin is increased, in the latter it remains unaltered, though the blood in other respects may be very similar.

But it is the white corpuscles which help us most. An increase of them, leucocytosis; a diminution, leucopenia; a normal number, and almost as much the proportion of the different varieties which is present, and in some cases the presence or absence of varieties which are not usually found in the blood, may all give us important information, not only for diagnosis, but also for prognosis.

I can best illustrate the use which is to be made of the information gained by blood examinations by recounting briefly some of the many cases in which, either in my own practice, in consultation, or by the kindness of my colleagues, I have examined the blood for diagnostic purposes, and by pointing out the method of interpretation in each case, which, as in

other methods of diagnosis, usually involves the weighing of *pros* and *cons*.

A case which made a deep impression on me at the time, was that of a young man on whom, on the 1st of the month, a cosmetic operation was performed. Everything went well until the evening of the 5th, when the temperature rose to 104°, and the patient developed a sore throat. The wound seemed to be all right, there was no pain; the patient was obviously very ill, but there was nothing on which a definite diagnosis could be founded, and the surgeon was naturally unwilling to believe that it was a septicæmia. The question put to me was, is this influenza (which was epidemic at the time), tonsillitis, or septicæmia? On the 7th I saw the case, when the temperature was about 103°, and examined the blood. The red corpuscles numbered 2,000,000 in the cubic millimetre, with about 30 per cent. of hæmoglobin, and the whites numbered 24,200, and were almost all polymorphonuclear. On the strength of this I assured the surgeon that it was septicæmia, and advised the opening up of the wound and the injection of antistreptococcic serum. Nothing was found in the wound, the temperature fell to 99·5 after the injection of the serum, but rose again and remained about 104°, in spite of a second injection, until death, which took place on the 9th. The interpretation of the blood examination was as follows:—Influenza was at once excluded by the leucocytosis, for that disease does not cause an increase in the number of leucocytes in the blood. Tonsillitis *might* have caused a leucocytosis of that size, though it is generally smaller, but could not possibly have caused the tremendous drop in the red corpuscles. The operation was a cosmetic one, and the patient had been in sound health before it was done, so that his red corpuscles were presumably about 5,000,000. The only things practically which could have caused a fall from five to two millions in a few days were hæmorrhage—which had not occurred—and septicæmia. The interpretation was amply justified by the progress of the case.

Another case of a similar character was that of a lady, who had been confined about five weeks before I saw her. It was found later that the house was in a very insanitary state. She went on well at first, but about ten days after the confinement

the temperature was found to be raised. She developed phlegmasia, first in one leg then in the other, but a gynæcologist who saw her found no local mischief in the uterus or pelvis, and the temperature began to fall. She got very pale, however, and exhibited the usual symptoms of anæmia. Her doctor thought she might be developing pernicious anæmia, and asked me to examine the blood. I found the reds to number 2,640,000, with 15 per cent. hæmoglobin, and a certain amount of poikilocytosis ; the leucocytes were 13,400, and mostly polymorphonuclear, the fibrin was increased. Pernicious anæmia was therefore out of the question—the reds were too numerous, the hæmoglobin too low, so that the colour index was only 0·3, instead of being at least 1·0 ; while the leucocytosis, though it was not very high, and the increase of fibrin pointed to an inflammatory condition. I therefore reported that the case was a septic one, though the apparent absence of local mischief was puzzling, and treatment was carried on on that line. The patient lived for three weeks, during which I saw her several times, and in a second blood examination found the reds lower and the whites higher. *Post-mortem*, we found that the uterus was not enlarged, but that on its posterior wall were two small ulcers, and starting from them a thrombosis, with purulent softening, of the right uterine vein. This passed into the iliac vein, down both femoral veins, up the vena cava, and into the pulmonary artery.

The converse of these cases was found in a man who had a double pleurisy with effusion, with a febrile but irregular temperature, a slight pericarditis, and an enlarged spleen. There was a vague history of rheumatism, but nothing else to point to a cause for the condition. Repeated blood examinations showed red corpuscles varying between 2,500,000 and 3,500,000, while the leucocytes were always below 5000. This last fact excluded rheumatism, where there is always a leucocytosis, depending in amount on the severity of the case, and excluded any septic element, while it made tuberculosis very probable. The man lived for two months, and then numerous miliary tubercles were found throughout the lungs and spleen, and a few in other organs. There is another point of interest in the case, and that is that this man had inflammation of three serous cavities, both pleuræ and the pericardium,

and, in spite of this, there was no leucocytosis. Had the inflammation been rheumatic or septic, affection of a single cavity would have sufficed to produce a marked increase of the leucocytes.

A companion to this case is one which I saw a few days ago, and which also illustrates some of the difficulties of blood diagnosis. A little girl, aged ten, was taken ill rather suddenly about a fortnight before I saw her, and her temperature had remained high (about 103°) all that time. She had some abdominal tenderness, slight diarrhœa, with dark motions, some bronchitis, and lay in a semi-comatose state. The spleen was not enlarged, and there were no rose spots. The blood was repeatedly tested for the typhoid reaction, but the result had been negative. When I examined her blood I found the red cells little altered and numbering 4,080,000, while the white cells were 9400. This of course excluded any suppuration or septic mischief or pneumonia, but left undecided the question whether the disease was typhoid or general tuberculosis. I therefore made a careful differential count of the leucocytes in the stained films, and found that the polymorphonuclears numbered 80 per cent., the lymphocytes 20 per cent. Now the number of leucocytes, 9400, was not high for a child, and I did not think it could be called a leucocytosis. But the normal percentage of lymphocytes in a child of ten should be about forty instead of twenty as in this case, and in typhoid, apart from complications, the tendency is for the lymphocytes to increase in number. It may also do so in tuberculosis, but much less frequently. On the whole, therefore, as the Widal reaction was negative, and as enlargement of the spleen and rose spots were both absent, I inclined to think the case one of general tuberculosis, and said so, but kept back in my mind the idea that there might be a commencing tubercular meningitis, as that is the one pure tubercular process in which a slight leucocytosis occurs. Two days after my examination the blood gave the Widal reaction strongly, and the case is now progressing favourably as a typhoid. The explanation of the relative leucocytosis, *i.e.* the increased percentage of polymorphonuclears, probably was that the bronchitis was more severe than one realised, and was producing this effect on the blood.

Another case illustrating the importance of leucocytosis was that of a girl, aged twenty-five, who was anæmic, slightly feverish, and had a markedly enlarged spleen and enlarged cervical glands. Her heart was enlarged, and there were slight systolic and diastolic murmurs. So far the condition might have been leucocythæmia, with a coincident cardiac lesion, but the blood was found to contain 18,000 leucocytes per cubic millimetre, and the vast majority of these were polymorphonuclears. There were no myelocytes, or nucleated red corpuscles, and no increase in the eosinophiles, as there would have been in a spleno-myelogenous leucocythæmia; and I hazarded the suggestion that the case was an endocarditis, which turned out to be correct.

I remember about six years ago passing two or three very unhappy days, which I might have avoided by a single blood examination. I was attending a family of children among whom measles had broken out. Three of them were attacked, and on the following day the eldest boy, whom I had not seen, so that he might have been feverish or not for all I knew, was taken ill with shiverings, cough, and high fever. At first I thought it was measles, as he had not had the disease, but the next day he was evidently very ill; the breathing had quickened, there was no rash, and no physical signs in the chest. Was it a severe aberrant form of measles, or a lobar pneumonia? The uncertainty lasted till the morning of the fourth day, when the familiar pneumonic signs showed themselves on one side, and the case went on satisfactorily. Had I counted the leucocytes on the first day I should probably have been able to make the diagnosis, for in measles there is no increase in their numbers; while in pneumonia, in 99 cases out of 100, there is a well-marked leucocytosis from the very first. There is probably, therefore, no disease where the blood-count will more constantly give information than in central pneumonia without physical signs.

But leucocytosis is just as important to the surgeon as to the physician. If surgeons would condescend to examine the blood systematically, they would find that, for instance, in appendicitis, a leucocytosis increasing day by day would sometimes give them warning of pus-formation before the temperature or physical examination gave any sign of it, while all other

septic conditions can be diagnosed from the blood more certainly than in any other way. The amount of leucocytosis present in malignant disease would give them an idea of the rate of growth and nature of the tumour, and of the existence or non-existence of metastases, and after its removal the reappearance of leucocytosis would warn them that the disease was recurring. In very mild and in very severe cases of pneumonia or other leucocytosis-producing diseases the blood gives a prognosis also. In obviously mild cases, where the patient easily overcomes the infection, the leucocytes are not increased, while in severe cases, where the infection easily overcomes the patient, there is no reaction and no leucocytosis. Such cases are doomed. In the great middle class, where there is leucocytosis, the actual amount of it depends on so many causes that it is impossible in most cases to draw a prognostic indication from it.

I might multiply cases which show the importance of leucocytosis indefinitely, but those I have narrated must suffice at present.

There are two diseases which are the very dust-bins of diagnosis, influenza in this country and malaria in America. It is worth remembering that neither of them cause leucocytosis, and that when we find a leucocytosis we cannot be dealing with uncomplicated influenza or malaria. I think all of us will be willing to confess that we are too ready to label anything that we are not sure of as influenza, and in another department of work I have scores of times seen febrile phthisis called by the other name, and have known all kinds of diseases, from appendicitis to tubercular meningitis, thrown into that dust-bin. In America the almost equal prevalence of malaria and typhoid leads to many mistakes, which might of course always be cleared up by a blood examination, the plasmodium on the one hand, and the serum-reaction on the other, being sufficiently definite. In this country we do not very often see malaria, and we see even less of the plasmodium ; for when a doctor finds that his patient has been in India recently, and has a febrile attack, his first instinct is to give him quinine, and it is not usually until a day or two afterwards that it occurs to him that it would be a good thing to have the blood examined. By that time, of course, the quinine has cleared the

plasmodium out, if it ever was there. If you want to make a diagnosis of malaria, examine the blood, fresh or in films, *before* treating the patient.

The varieties of leucocytes present in the blood may give us useful information, and this must of course be sought in stained films. In passing, I may say to men who have no time for complete blood examinations, that they can learn more from stained films than from any one other method, and that if to that they add the enumeration of leucocytes, it will often be unnecessary to count red corpuscles, to estimate hæmoglobin, or even to examine a fresh film. With practice one can make a fair guess at the amount of hæmoglobin in the blood as it flows, and if one finds the red corpuscles normal in shape in the films, they are not as a rule greatly diminished in number; whilst by spreading films always as nearly as possible of the same thickness, one gets to know how near to one another the corpuscles should lie, and one can at once detect any considerable alteration in the number of leucocytes. The percentage of the different varieties of leucocytes can only be ascertained in films, for it is thoroughly misleading to attempt to estimate them from the number counted in the leucocytometer, though it is often stated in text-books that this is sufficient. One rarely counts more than a hundred leucocytes in that instrument, often not so many, and it is necessary to count at least thrice as many, and for real accuracy five hundred or a thousand. Further, in the leucocytometer one has not the staining reactions to guide one, so that a myelocyte looks just like a large lymphocyte, and an eosinophile like a "neutrophile." It is to be remembered, moreover, that the combination of eosin and hæmatoxylin is not a sufficient stain for diagnostic purposes; some combination of basic and acid anilin stains such as the triacid stain or eosin and methylene blue, must be used.

The following cases will illustrate the information to be obtained from stained films. About a year ago I saw a child with a large abdominal tumour on the left side. The doctor in charge of the case believed that this tumour was the spleen, and in shape and relations it was certainly very like it. He had examined the blood, had found the red corpuscles about 3,000,000, and the leucocytes between 30,000 and 40,000, if I remember aright. He had examined some films also, and

thought that from them he got evidence that the disease was leucocythæmia. I saw the case, and as I knew his enumerations were likely to be accurate, did not repeat them, and merely made some films. When I examined these I found that there were no myelocytes and no eosinophiles, but that the majority of the leucocytes were polymorphonuclear neutrophiles, and the rest lymphocytes. It was therefore a leucocytosis, and not a leucocythæmia. I gave the opinion that the tumour was a sarcoma, probably of the kidney. At the *post-mortem* it was found to be a sarcoma of the suprarenal capsule, an extremely rare condition.

Another case which I did not see, and where the diagnosis, so far as I was concerned, was made from the films alone, was that of a gentleman who had been out of health for some time, but had not consulted his doctor. He became suddenly very ill, and his doctor found that his spleen was much enlarged and that his temperature was 105°. He sent for my friend, who afterwards gave me the films, but the patient was moribund, and died soon after. The films gave evidence of some anæmia; there were a few nucleated red corpuscles present, and in the three coverglasses which I received *every* leucocyte was a lymphocyte, and they were evidently increased in number. The explanation I gave was that the case was one of lymphatic leucocythæmia, in which the spleen is enlarged, the percentage of lymphocytes in the blood is often 98 or 99, and there is always some anæmia and a few nucleated red corpuscles; that the man had been attacked by some severe organismal infection, which had called out of his blood the few polymorphonuclear cells which were there, and that he had died before he had time or opportunity to manufacture a leucocytosis.

A very interesting series of observations is beginning to be made with regard to the eosinophiles. It has long been known that they are increased in the blood and sputum in asthma, and that one can by that means distinguish true asthma from cardiac and renal dyspnœa. They are also increased in some skin diseases, notably in pemphigus and in dermatitis herpetiformis. In one case of the latter disease I found them to form 16 per cent. of the leucocytes present (the normal percentage is under 5), and have been able to show that two cases,

supposed to be of that disease, were not so, by finding the percentages to be 4 and 5 respectively. But the disease which gives the highest percentage of eosinophiles is trichinosis—for some unexplained reason—and there they have been found in one case to make up 68 per cent. of all the leucocytes present. At first sight nothing seems more unlikely than that one should be able to diagnose such a disease by a peculiarity in the leucocytes, but the observation is quite recent, and it is an evidence that there is still much to be found in the blood if it be carefully and systematically examined.

The cases which I have chosen to illustrate my remarks have been selected in order to show that there is no branch of our profession which can afford to neglect the information to be got in this way. I do not mean, of course, that every patient who has a whitlow or a prolapsus uteri should have their blood examined, nor that any other means of diagnosis should be neglected, but I do plead for a systematic examination in all serious cases where there is any doubt about the diagnosis. The blood will often give positive information, and in many other cases will give negative information, which is often just as valuable. Those of the members of the Society who are familiar with the subject will of course realise that I have merely touched the fringe of it. My desire has not been to make an exhaustive exposition, but to stimulate interest and encourage observation.

THE EXCRETION OF UREA BY THE SKIN IN HEALTH

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DURING an appointment as house physician to Dr Wyllie at the Edinburgh Royal Infirmary, I admitted into the wards a woman who had been found by her neighbours in a state of unconsciousness, with "convulsive fits" affecting the body and limbs, and frothing at the mouth. She was conscious at the time of admission, but the extremely high tension of her pulse at once arrested attention; and this, combined with the history which she was able to give—namely, that she had lately been suffering from headache, dimness of vision, and vomiting; that

she had had similar "fits" two months before; and that she had been intemperate for many years—made it advisable to examine the urine, nine ounces of which were secured for investigation, with the following result:—Colour, dark amber; specific gravity 1014; reaction acid; phosphates abundant; albumin present (·5 gr. per oz.); no blood or sugar; urea only 2·5 grains per oz.; and numerous tube-casts, fatty, granular, and hyaline. The diagnosis was uræmia from Bright's disease. The patient was put to bed between blankets and hot bottles, 120 grains of compound jalap powder were administered in one dose, the kidneys were dry cupped, and a strict arrowroot diet was meanwhile ordered. During the afternoon she had two slight uræmic convulsions, and towards evening she became unconscious and delirious, constantly moaning in a rapid monotonous manner, "Oh, dear! Oh, dear!" . . . Her breathing was rapid (36 per minute) and panting in character, and the breath urinous in odour. The bowels had acted freely four times since the administration of the jalap, but the skin remained dry and harsh notwithstanding the use of the blankets and hot bottles, and no urine had been passed. The bowels had thus done their share in the elimination of the pent-up urea and other toxic products in the body, but the skin like the kidneys had struck work. One-eighth of a grain of nitrate of pilocarpine was injected hypodermically at 8 P.M., and again at 9.30 P.M., but without any effect upon the skin, which still remained dry and harsh. Her constant moaning was now causing much annoyance to the other patients in the ward and preventing them from sleeping, and so at midnight she was given a hot pack. In half an hour, by which time she was *perspiring profusely, the delirium suddenly ceased.* In an hour the pack was removed, and during the rest of the night she had snatches of sleep, passed some urine and fæces in bed, and gradually became conscious. She made an uninterrupted recovery, and was discharged from hospital in six weeks. In this case the turning point of the uræmia coincided exactly with the induction of free perspiration.

Last year a male dement, who for twenty years had been boarded out in the country as a harmless lunatic, was returned to the Edinburgh Royal Asylum on account of a "difficulty in micturition," which could not be properly attended to in his country abode. On admission he was slightly delirious, the

heart's action was weak and laboured, and there were asthmatic and bronchitic sounds over both lungs. There was no bladder dulness. Next morning he was much worse, being unconscious and delirious, the breathing asthmatic in character, and the breath suspiciously urinous in odour. As no urine had been passed since admission a catheter was introduced into the bladder, which contained only 2½ oz. of urine, and this on examination revealed a trace of albumin, 4½ grains per oz. of urea, and fatty tube-casts. The condition was evidently uræmia from Bright's disease superadded to the old-standing dementia. Free perspiration was induced by means of hot bottles, the sweat containing numerous needle-like urea crystals, and yielding the characteristic plate-like crystals of urea nitrate with nitric acid. It was considered unsafe to use a hot pack owing to the feeble state of the patient's heart. Notwithstanding the employment of other remedies, the man died within twenty-four hours of admission, he having excreted during this time only 8 oz. of urine containing in all 36 grains of urea. At the *post-mortem* examination the right kidney weighed 3 oz., and was markedly cirrhotic. The left kidney weighed 7 oz., and presented the appearances of "large white kidney," the cortex being markedly pale and swollen, and exhibiting on microscopic examination extensive catarrhal and fatty changes in the epithelium of the urinary tubules. The above two cases illustrate the fact which is well recognised and taken advantage of by physicians in their practice, namely, that in diseases in which the elimination of the urinary urea is impaired, the skin may excrete relatively large quantities of urea and other products, and so greatly relieve the action of disabled kidneys.

One might reasonably infer that in health the skin has considerable powers of urea excretion, but the teaching of the great majority of physiologists is distinctly opposed to this idea. Thus Foster (1) states that the solids of the sweat amount to 1% to 2%, one-third of these being inorganic (chiefly sodium chloride), and two-thirds organic (chiefly volatile fatty acids, neutral fats, and cholesterin, also a trace of urea). Foster says: "Apparently some small amount of nitrogen leaves the body by the skin as a whole, but this is probably supplied by the sebum or by the epidermis." Landois and Stirling (2) estimate the solids of the

sweat at 1·2%, of which ·3% is inorganic and ·9% organic, the latter including "a trace of albumin and urea." Howell (3) states that the organic constituents of the sweat are present in "mere traces," and that "urea, uric acid, creatinin, . . . and albumin" are said to occur when sweating is profuse, but that "under perfectly normal conditions it is obvious that the organic constituents are of minor importance." On the contrary, however, Argutinski (4) finds that a considerable quantity of urea may be eliminated in the sweat; Leube [quoted by Landois and Stirling (2)] says that when sweating is profuse the urea in the urine may be diminished; and Waymouth Reid (5) states "there is no doubt that urea is present in the sweat of man," and that "the nitrogen excreted by the skin may amount to 4·7% of that by the urine, and hence may have to be taken into account in some experiments on nitrogenous metabolism."

The following observations, made independently upon myself, lead me to believe that in health, under ordinary physiological conditions, the skin has greater potentialities of urea excretion than is generally supposed. My attention was originally directed to this subject in the course of an experiment which at the time was being undertaken for an entirely different object than the investigation of the cutaneous urea, and in which on a fixed mixed diet the excretion of the urinary urea presented certain marked variations which, on an explanation being sought for them, were found to correspond to and to be apparently dependent upon variations in the amount of perspiration. The following experiments serve to indicate that there is in health a direct quantitative relationship, not only between the water of the urine and that of the sweat, as is well known, but also between the urinary and the cutaneous urea.

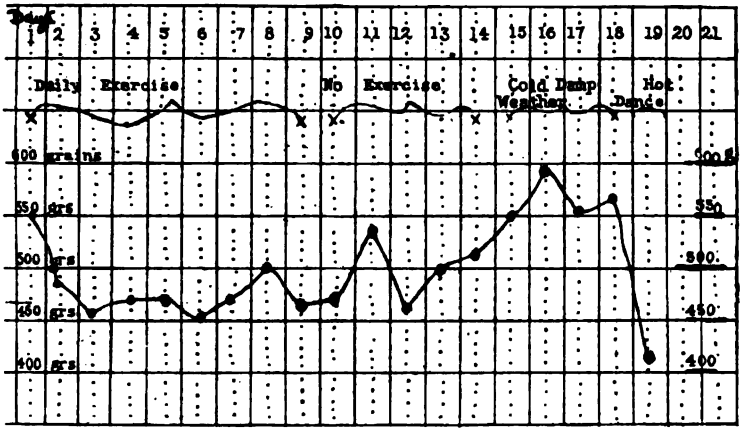
Experiment I.—To estimate the daily excretion of the urinary urea on a fixed mixed diet. A fixed mixed diet was maintained for nineteen days, the same breakfast, the same lunch, the same dinner, etc., being rigidly repeated day by day during the period, the quantities of the various foodstuffs and liquids taken at each meal being carefully weighed and measured beforehand. The twenty-four hours' discharge of urine was collected day by day, mixed and

measured, and the urea was estimated by the sodium hypobromite method, Gerard's ureameter being the apparatus used. On several days the quantity of urea excreted during different periods of the day was also estimated. During the experiment the body-weight (200 lbs.) remained constant, the body-temperature varied within the limits of health, and the bowels were perfectly regular. Daily notes were also made of the amount of exercise taken, and of the state of the weather.

Table I.—Daily excretion of urinary urea on a fixed mixed diet. Body weight 200 lbs. (Approx. 90 kilogs.)

Day.	Urine (Oz.).	(Cub. Cent.).	Urea (Grains).	(Grms.).	Remarks.
1.	39	(1107)	546·0	(35·4)	One hour's daily exercise (tennis).
2.	39	(1107)	479·7	(31·1)	do.
3.	50	(1420)	460·0	(29·8)	do.
4.	72	(2044)	475·2	(30·8)	do.
5.	52	(1476)	478·4	(31·0)	do.
6.	43	(1221)	454·3	(29·5)	do.
7.	60	(1704)	474·0	(30·7)	do.
8.	70	(1988)	504·8	(32·7)	do.
9.	60	(1704)	470·1	(30·5)	do.
10.	52	(1476)	478·4	(31·0)	No daily exercise.
11.	59	(1675)	542·8	(35·2)	do.
12.	70	(1988)	462·2	(30·0)	do.
13.	57	(1618)	501·6	(32·5)	do.
14.	60	(1704)	528·0	(34·2)	do.
15.	91	(2584)	555·1	(36·0)	Cold N.E. wind. Snow and sleet. Walk 12 miles.
16.	90	(2556)	594·0	(38·5)	Cold N.E. wind. Snow and sleet. No exercise.
17.	76	(2158)	562·0	(36·4)	Cold winds. Walking exercise.
18.	86	(2442)	567·6	(36·8)	Cold winds. Walking exercise.
19.	34	(965)	417·0	(27·0)	Dance with free perspiration on night 18th to 19th.

The facts of this experiment are more graphically shown in the following chart.


1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17 18 19 20 21
Daily Exercise
No Exercise
Cold Damp Weather
Hot Dance
600 grains
550 grs
500 grs
450 grs
400 grs

1. The most striking fact of this experiment is the big fall in the urea on the 19th day. It will be noticed that during the preceding eighteen days the differences in the successive readings of the urea from day to day average about 31 grains, the smallest difference being 3 grains between the 4th and 5th days, and the largest difference being 80 grains between the 11th and 12th days. On the 19th day (417 grains) there is a sudden fall in the urinary urea of 150 grains, as compared with the reading for the 18th day (567 grains), that is a fall nearly five times greater than the average daily difference during the previous eighteen days. What was the explanation of this big fall in the urinary urea? On the night between the 18th and 19th days, the writer was present at a dance. The room was warm and confined and the dance vigorous, and the rapid moving about for some five hours in a warm atmosphere induced very free perspiration. On the 19th day the urine registered only 34 oz. as compared with the 86 oz. of the 18th day, and the natural explanation of the concomitant big fall in the urinary urea would seem to be that the free perspiration during the dance had carried off not only a considerable quantity of water, but also of urea which would otherwise have been excreted by the kidneys. It was this big fall in the urinary urea on the 19th day, and its apparent ex-

planation, which first drew my attention to a possible relationship between the urinary and cutaneous urea in health. It led me to look more carefully into the variations of the urinary urea which had already taken place during the previous eighteen days of the experiment, and the following additional facts became evident.

2. It will be observed that during the first nine days I took one hour's daily exercise at tennis, this usually serving to produce a certain amount of free perspiration. During the next five days (10th to 14th inclusive) I took no exercise. It will be noticed that the total quantity of urinary urea excreted during these five days with no exercise (qty. = 2511 grains) was greater than the total amount excreted on *any five* of the preceding nine days with exercise (2483 grains = the sum of the five largest readings, namely on the 1st, 8th, 2nd, 5th and 4th days). The explanation of this seems to me to be that on the days with exercise, the free perspiration induced served to carry off from the system a certain amount of urea which would otherwise have been excreted by the kidneys, whereas on the days with no exercise there was no such perspiration induced and consequently the urinary urea read higher.

3. A third observation may be made which confirms the above and emphasises the effect of the weather upon the action of the skin. During the four days from the 15th to 18th inclusive there was a spell of bitterly cold and damp weather, the prevailing winds being north-easterly, and accompanied by showers of sleet and snow. During these four days the excretion of urinary urea reached its maximum, the total quantity for the four days (= 2278 grains) far exceeding the quantity excreted on even the four of the preceding fourteen days with the largest readings of urea (2120 grains = sum total for 1st, 11th, 14th, and 8th days). As is well known both cold and damp diminish perspiration, and, as is well shown in Table I., increase the urine. The high readings of the urinary urea on the four cold days would apparently be explained by a concomitant fall in the cutaneous urea, due to the diminution of perspiration.

4. Finally it will be observed that on one of the four cold days, namely the 16th, the urinary urea reached its absolutely highest reading, viz. 594 grains. On this day no exercise was

taken, and perspiration was less than on the other three cold days on each of which walking exercise was taken.

An important point in the above experiment was its duration, for it is obvious that no conclusions could have been come to during the first ten days, and practically the results became evident only during the latter half of the period when, by a fortuitous succession of circumstances, there were observed the effects, first of the absence of exercise, secondly of cold damp weather, and lastly of a vigorous hot dance. The rise in the urinary urea during a period of five days without exercise as compared with the preceding period of nine days with exercise; a still further rise in the urinary urea during a period of four cold damp days; the maximum rise in the urinary urea on the only one of the four cold days on which no exercise was taken; and finally the marked fall in the urinary urea after a vigorous hot dance, all indicated that the urinary variations were dependent upon the concomitant state of the perspiration, and the natural inference was that urea had been present in the sweat and had appreciably modified the amount in the urine. This conclusion, as will have been seen, was arrived at indirectly, not by a direct examination of the sweat itself, upon which method alone the proof must necessarily rest. Before describing the experiments upon the investigation of the sweat, experiments which I may here state are entirely in support of the above conclusion, it will be advisable to consider whether the variations in the urinary urea in Experiment I. could be explained in any other way than by variations in the amount of perspiration.

Firstly, the variations were not due to diet, which was the same in nature and in amount day by day, being carefully weighed and measured for each meal.

Secondly, it might be objected that muscular exercise, heat and cold *per se* by their influence upon metabolism produced the variations in the urinary urea. Without raising the intricate problems of the effect of exercise and temperature conditions upon metabolism, and without denying the possibility of intrinsic specific variations in the rate of metabolism day by day, I would merely point out that in the above experiment the variations in the urinary urea coincided so remarkably with the varying states of functional activity of the skin, howsoever these were produced, as to at once suggest that the state of

the perspiration afforded a sufficient explanation of the urinary variations. For instance it is not supposed by physiologists that muscular exercise diminishes proteid metabolism, and yet the fall of 150 grains in the urinary urea on the 19th day, after five hours of vigorous warm dancing would almost enforce such a conclusion, unless one recognised that the urea had had another outlet, namely, by the skin. Again on three of the four cold days a walk of from ten to twelve miles was indulged in. Now it is estimated that one covers much the same distance in a five hours' dance, so that much the same amount of exercise would be taken and energy expended at the warm dance as during the cold walk. And yet how different are the readings of the urinary urea for the two events. Diet being constant and exercise being similar in amount though different as regards the degree of accompanying perspiration, the natural explanation of the difference in the urinary urea was that the skin eliminated urea and did so in direct proportion to the degree of its activity.

Thirdly, it might be objected that in Experiment I. no investigation was made of any elimination of urea by the bowel, and that possibly the urinary variations were really due to variations in the urea in the fæces. In a man of 70 kilogrammes weight with complete nutritive equilibrium, it is estimated by Burdon Sanderson (6) that, of 15·5 grammes of incoming nitrogen 14·4 grammes are excreted in the urine and 1·1 grammes in the fæces. C. Voit (7) estimates that, in a man of 70 kilogrammes weight with nitrogenous equilibrium, out of 19·5 grammes of incoming nitrogen 17·4 grammes are excreted in the urine and 2·1 grammes in the fæces. No estimate is made in either of the above calculations of any nitrogen escaping by the skin, and one would infer that the latter must be trivial in comparison with the small proportion of nitrogen in the fæces. Accordingly it would seem that a valid objection might be taken to the interpretation of Experiment I., in which no investigation was made of the possible presence of urea in the fæces. From the following experiment, which was undertaken for this purpose, it would appear that while the fæces may be a more important outlet for *nitrogen* than the sweat, this does not apply to *urea*, which is practically absent from the fæces in health.

Experiment II.—To estimate the daily urea in the urine and fæces on a fixed mixed diet. A fixed mixed diet was taken for fourteen days, and, although it was not precisely the same as that taken during Experiment I., I may mention it here as it was typical of the constant diets employed when these were necessary. It consisted of in the twenty-four hours —water 55 oz., milk 35 oz., cream 2¼ oz., eggs 14 oz., bread 12 oz., Madeira cake 5 oz., sugar 3½ oz., butter ¾ oz., potatoes (cooked) 4 oz., cabbage (cooked) 4 oz., arrowroot 4 oz., corn-flour 1 oz., orange 4 oz., tea ½ oz., coffee ½ oz., salt and pepper. Based upon the figures given for the composition of the food-stuffs in Burney Yeo's "Food in Health and Disease" (8), an approximate estimate of the above diet in terms of the alimentary principles would be—proteids 5½ oz., fats 4½ oz., carbohydrates 17 oz., salts 1⅓ oz., and water 115 oz. This is a fairly liberal and proportionate diet for a person of 200 lbs. weight doing active work. During this experiment the urinary urea was estimated daily as in Experiment I., and, as before, it showed variations which were accountable for by the state of the perspiration. There was one motion per diem, and the stool was carefully analysed for urea for seven days, but with negative results. Several methods were tried. The freshly passed stool was extracted for twenty-four hours in alcohol, and the alcoholic extract yielded no urea, either before or after evaporation. Extracts in water and in acidulated water similarly yielded negative results, the tests employed being those to be presently mentioned. One may conclude that for practical purposes urea is absent or negligible in the fæces in health. The nitrogen in the fæces in health comes mainly from undigested foodstuffs. There being no outlet for the urea in the fæces, metabolism being presumably uniform and diet being constant, it follows that the explanation of the day-to-day variations in the urinary urea was to be sought in a concomitant excretion of urea in the skin.

The following experiments were undertaken to directly investigate the capacity of the skin to excrete urea.

Experiment III.—To compare the composition of the sweat as regards urea before and during the production of free perspiration.

1. *Effect of Muscular Exercise.*—Two clean measuring

glasses were taken, and into each was put half an ounce of distilled water and a small piece of purified linen. After the pieces of linen had been allowed to steep for some minutes, the water in each glass was tested for the presence of urea, the result, as was expected, being negative. The forehead was then carefully laved with pure water and allowed to dry, and at intervals during the next three-quarters of an hour, the piece of linen from one of the glasses was gently passed over the forehead, being held for the purpose in clean dressing forceps, and then steeped as before in the water in its glass. The water was then tested for the presence of urea, a mere trace being found but sufficient to show that urea exists in the ordinary "insensible" perspiration. Free perspiration was then induced by muscular exercise, a hard single at tennis being indulged in for three-quarters of an hour with a sweater on in a strong sun. The piece of linen from the other glass was at intervals during the game gently rubbed over the brow with forceps as before, and between the intervals steeped in the water in its own glass. This water, containing the sweat of "sensible" perspiration, was then tested for the presence of urea, whose reactions were given with much greater readiness and distinctness than in the previous case. The tests employed for the detection of urea in all cases were the following:—

(1) *Sodium hypobromite test.*—A few drops of the water to be tested were allowed to fall into some sodium hypobromite solution, when an effervescence occurred due to liberation of nitrogen when the latter was present. This was a rough preliminary test, employed merely to give a rapid clue to the presence of nitrogen. It was, of course, unreliable as a test for urea, because human sweat may contain, according to some authorities, other nitrogenous bodies, *e.g.* uric acid, creatinin, and an albuminous body from the sebum.

(2) *Nitric acid test.*—Half of the water to be tested was evaporated almost to dryness in a clean porcelain capsule, and allowed to cool. Strong nitric acid was added to the residue, when the characteristic colourless plate-like crystals of urea nitrate were recognised under the microscope when urea was present.

(3) *Biuret test.*—The remaining half of the water to be

tested was evaporated to dryness, when any urea present became represented by its decomposition product, biuret. Liquor potassæ was then added to the residue and also a drop of dilute cupric sulphate solution, when in positive cases a rose-pink colour appeared showing that urea had been present.

2. *Effect of Hot Baths.*—The preceding experiment was repeated step by step, the only difference being that free perspiration was induced not by muscular exercise but by sitting in a hot bath (Temp. 105° F. or 40·5° C.) up to the shoulders for three-quarters of an hour. The results were similar, the "insensible" perspiration of the brow before the bath yielding just a trace of urea, and the "sensible" perspiration during the bath containing urea in abundance. Similar observations were made on other parts of the body besides the forehead, namely, on the arms, legs, palms and soles, the part under observation at the time being kept above water. Similar results were obtained as in the case of the forehead.

These experiments on the effect of muscular exercise and hot baths upon the composition of the sweat prove directly that the skin in health excretes urea, and that whenever it breaks into free perspiration its excretion of urea at once rises.

Argutinski's experiments (4), with which I was unacquainted at the time, were somewhat similar to my own. He found that the effect of vapour baths and of muscular exercise in the form of long walks and mountain climbs was to cause a considerable quantity of urea to be eliminated in the sweat.

The quantitative estimation of the total sweat, as it is well known, is a matter of great practical difficulty. Possibly the least fallacious method is the hygrometric, in which the subject sits in a respiration chamber breathing through tubes to the exterior, the amount of moisture given off by the skin being estimated from the readings of hygrometers placed in the currents of air which enter and leave the chamber. This method, however, is obviously inapplicable for the estimation of urea. Other methods are to take exercise in a special suit of clothes and to make extracts of the clothes; or, again, to enclose the body, with the exception of the head, in a large caoutchouc bag or metallic vessel, and to raise the temperature of the

surrounding air by placing the bag or vessel containing the body in say a hot bath, when the sweat collects in the bag or vessel and is simply measured. These methods are not very reliable for the estimation of the urea in the sweat, even although urea is a soluble substance, because we cannot be sure that a considerable quantity of urea does not remain on the skin instead of trickling off with the water. As has been already mentioned, in profuse sweating in uræmia crystals of urea may form on the skin. Probably the hot air method followed by a washing down of the body with a known quantity of pure water would be the most accurate way of estimating the total cutaneous urea. I have not attempted this method, not having the necessary apparatus, but I have made estimations of the amount of urea given off in a definite time from particular areas of the skin, and although it would be fallacious to base calculations of the total cutaneous urea on such local computations—owing to the varied activity of different regions of the skin—they suffice to give one a clearer conception of this special excretory power of the skin in health.

Experiment IV.—To estimate the amount of urea eliminated in a given time from special areas of skin. The following group of experiments was performed on a fixed diet as before. Observations were made on the brow, forearm, leg, palm and sole. Sweat in sufficient quantities for analysis could not be collected from the palm or sole. In the case of the other areas the results merely differed in degree, the brow being more active than equal areas of the leg or forearm. It will suffice to describe the results in the case of the brow alone. The experiments were repeated many times and at various periods of the day.

1. *Effect of Hot Baths.*—The brow being thoroughly cleansed and dried, I sat in a hot bath (Temp. 105° F. or 40·5° C.) up to the shoulders until free perspiration was established—this taking from five to fifteen minutes on the various occasions. Five cubic centimetres of sweat were then collected from the brow by simply allowing the drops to trickle into a thin glass test-tube. The 5 c.c. of sweat were usually collected in from five to ten minutes, but sometimes fifteen or more minutes were necessary according to the activity of the perspiration. The 5 c.c. of pure sweat, on estimation with the

ureameter, contained never less than ·1% urea, and never more than ·2% urea, the lower reading being the more frequent. In several of the experiments the free perspiration was maintained for an hour, and analysis of the sweat collected at the beginning, middle, and end of the hour showed the same percentage of urea to be maintained throughout. In many of the hot bath experiments the urea in the urine was estimated immediately before and immediately after the period of free perspiration, and in all cases there was a drop in the percentage of the urinary urea after the perspiration, this varying from ·1% to ·3%, and thus indicating a direct quantitative relationship between the urinary and the cutaneous urea. It was also noticed that the lower readings of the cutaneous urea were more common in the mornings and the higher readings in the evenings. In Experiment I., as before mentioned, estimations were made on several days of the quantity of urea excreted in the urine at different periods of the day on a fixed diet, and it was found that as a rule much more urea was excreted in the evenings than in the mornings. Thus on the eighth and ninth days, which it is convenient to divide into three periods of eight hours, the readings of the urinary urea were as follows :—

Period of Day.	8th Day.	9th Day.
10.30 P.M. to 6.30 A.M.	132·0	129·5
6.30 A.M. to 2.30 P.M.	132·0	125·4
2.30 P.M. to 10.30 P.M.	240·8	215·2
	504·8 grains.	470·1 grains.

It will be seen from this table that during the evening period of eight hours, this covering the time of the chief nitrogenous feeding as well as the end of the day's work, the excretion of urea by the kidneys was much more active than during the eight hours of sleep or than during the earlier half of the day. The fact that the urea in the sweat frequently exhibited a similar diurnal variation, would indicate that cutaneous excretion like renal excretion participates in the general metabolic processes of the body, being more active in the later period of the day, when katabolism predominates, than in the night and early hours, when the anabolic phase is in the ascendant.

2. *Effect of Muscular Exercise.*—The estimation of the cutaneous urea in the sweat of exercise was a specially interesting problem. Observations were mainly confined to the region of the forehead. What one particularly wished to ascertain was whether there was any difference in the percentage of urea in the sweat of exercise as compared with the sweat of hot baths. It was difficult to arrange the experiment in such a way as to ensure a fair comparison. For instance, as the result of bicycling, it was found that the sweat of the brow contained from ·2% to ·3% urea, but this estimation was open to the obvious objection that owing to the rapid movement in the air, the more rapid evaporation of the sweat, and the more tardy collection of the fluid, one had really to do with a more concentrated sweat solution, the result not of muscular exercise but of the conditions of the experiment. Again, as the result of several bouts at dumb-bells and Indian clubs in a confined room, it was found that an equal quantity of sweat could not be collected even in double the time taken in the case of the hot baths. In order to collect from the brow 5 c.c. of the sweat of exercise in five, ten or fifteen minutes, it was necessary to call in the aid of the hot bath. Two methods of procedure were tried. (1) Vigorous exercise, either on the bicycle or at dumb-bells and Indian clubs, was indulged in for a period immediately before the bath; while active perspiration from the exercise was going on, the brow was carefully cleansed, and perspiration was maintained by sitting in a hot bath until sufficient sweat had been collected for analysis. (2) Free perspiration was initiated by sitting in a hot bath, and, the brow being dried, was maintained by continuous exercise at dumb-bells and clubs until sufficient sweat had been collected. Where the exercise had been in the form of dumb-bells and Indian clubs, it was found that—whether the exercise immediately preceded or immediately succeeded the bath—the percentage of urea in the sweat was practically the same as in the case of the hot baths without exercise; that is, usually ·1%. In the case of bicycling, however, the percentage was higher, usually ·2%, thus indicating that in vigorous exercises like bicycling, in which the bulky lower limbs are more extensively employed, the sweat contains a larger portion than usual of the excretory urea. In one experiment I

bicycled thirty miles in three hours, and made simultaneous estimations of the urea in the sweat and urine immediately at the close of the exercise, and then at periods of twelve, twenty-four and thirty-six hours afterwards. A hot bath was employed on each occasion to aid free perspiration. The readings were as follows :—

Time of Observation.	Urine.	Sweat.
(1) at close of ride	2·3% urea	·2% urea
(2) 12 hours afterwards	3·0% „	·1% „
(3) 24 „ „	3·3% „	·1% „
(4) 36 „ „	4·0% „	·1% „

It will thus be noticed that at the time of free perspiration from active exercise the percentage of urea in the sweat was increased and that in the urine about its normal, and that during the thirty-six hours following the exercise the percentage of urea in the sweat resumed its ordinary level while that in the urine rose. A rise in the urinary nitrogen for a day or two after severe exercise has been noticed by several observers.

Conclusions.—The preceding experiments indicate that in health urea, the chief end-product of proteid katabolism, is excreted by the kidneys and skin and not by the bowel. While it is obvious that the main function of the urine is to remove the urea from the body, and that the chief object of the perspiration is by its evaporation to form an outlet for the heat set free within the body, an appreciable quantity of urea is eliminated by the skin daily. The skin's capacity in this direction in health is considerable, and the chief observation to be derived from the above experiments is that the excretion of urea by the healthy skin at once rises as soon as free perspiration sets in, and that it remains high as long as the skin continues to act freely. A considerable quantity of urea is thereby eliminated by the freely acting skin, varying in particular areas but forming in all an amount which suffices to distinctly lessen the output of urea in the urine. A quantitative relationship thus exists between the urinary and the cutaneous urea.

The bearing of these observations on problems of metabolism, and especially on the relation of metabolism to muscular work,

is apparent. It is now generally agreed by physiologists that the main and immediate source of muscular energy is the metabolism of carbohydrates and fats. This long recognised opinion has always been difficult to reconcile with the fact that hard muscular work is best done on a diet containing proteids. A brief review of physiological inquiry on this subject reminds us that Liebig first promulgated the doctrine that proteid metabolism was the source of muscular energy. Then followed the experiments (1) of Fick and Wislicenus, who found that the urinary nitrogen was not increased by muscular work; (2) of Parkes, who found that the urinary and the fæcal nitrogen were not increased by muscular work; and (3) of Pettenkofer and Voit, who found as the result of muscular work no increase in the urinary urea, but a great increase in the respiratory carbonic acid. Consequently non-nitrogenous metabolism came to be regarded as the source of muscular energy. Next followed the experiments of Argutinski, who stated that the total nitrogen in the urine, fæces, and sweat was increased by muscular exercise, and that the increase in the proteid metabolism sufficed to account for the energy of the work done. It is contended, however, that Argutinski did not supply enough fat and carbohydrate in his diet, and that consequently there was proteid waste. The general opinion of physiologists at present is that, if the food supply be sufficient in quantity and suitably proportioned as regards the alimentary principles, muscular energy springs from the oxidation of glycogen and fat and not from proteid metabolism. It is difficult to accept this opinion without modification if we are to believe the latest views of the nature of metabolism (9). Putting the matter succinctly, we are to regard the protoplasm of cells as consisting on the one hand of bioplasm or "organised" proteid which is the only living constituent of the cell, and on the other hand of several non-living constituents lying in contact with the bioplasm and originally derived from the food. The non-living constituents include, in addition to water and salts, the "unorganised" proteid which is the immediate source of nourishment of the bioplasm, and the fat and carbohydrate which are regarded as the immediate source of the bodily energy. The bioplasm of the cell by "contact" action regulates all the physico-chemical changes which take place in the non-living

constituents, much in the same way as organised ferments determine the fermentative changes in the substances with which they are in "contact." Schäfer says "metabolism may occur both as a splitting-up and oxidation of the molecules of living tissue or bioplasm, and as a splitting-up and oxidation both of unorganised proteid and of non-proteid materials outside of but in contact with the molecules of bioplasm." The proteid of the tissues, whether organised or unorganised, is believed to split up in the process of its metabolism into a nitrogenous part which becomes urea, and into a non-nitrogenous part which may become fat or carbohydrate but is eventually oxidised into carbonic acid and water, and so may be a source of bodily energy. Hence one is forced to the conclusion that no metabolism can occur at all without an accompanying metabolism at least of the nitrogenous bioplasm which regulates the changes in the other constituents of the cell. The bioplasm in dominating metabolism is itself metabolised in the process; and this seems to be the reason why muscular work, which is a powerful metabolic stimulant, is best done on a diet containing proteid, although the immediate source of its energy is the metabolism of non-nitrogenous matter. The proteid in the food is necessary to make good the wear and tear and exhaustion of the bioplasm. If the preceding views be correct, then it follows that metabolism during muscular exercise involves not only the metabolism of glycogen and fat, but also the metabolism of bioplasm and possibly also of unorganised proteid. Hence in exercise there must be an increase in the excretion of urea, whose quantity in health varies directly with the total nitrogen excreted from the body, and so may be taken as a reliable index of the total nitrogenous metabolism. The additional excretion of urea from exercise may be small in quantity, for it is stated by Voit that the proteid of the tissues is metabolised daily only to the extent of about 1% of its substance, and it may not be apparent at the time in the urine but may occur a day or two after the exercise.

In the case of my own experiments I was careful to adopt a mixed diet which was sufficient to insure, as the sequel showed, (1) maintenance of body-weight, (2) maintenance of body-temperature, and (3) a plentiful supply of fuel for the bodily energies. I found that when free perspiration was

induced the percentage of urea in the sweat rose and remained high as long as the perspiration continued. This was most marked when perspiration was induced by those forms of exercise which involved the free use of the bulk of the muscles of the body, *e.g.* bicycling and dancing and running.

In my opinion this rise in the urea of the sweat of exercise is the direct proof of an increase in proteid metabolism during exercise. The older observers who did not investigate the sweat came to contradictory conclusions as to the rôle played by proteid metabolism during exercise. During active exercise on a fixed diet the urinary urea gives variable readings according apparently to the degree of concomitant perspiration. The cutaneous urea is always increased; the urinary urea may also be increased, it is more often unaffected, and sometimes it is decreased, but only when sweating has been profuse, and so has carried off an extra amount of urea from the body. For a day or two after active exercise the urinary urea may rise. This rise may be due to a continuance of the excretion of the additional urea in the system, and to a persistence for a longer or shorter time of the stimulating effect of exercise upon the bioplasm of the body. It is associated by Noël Paton (10 and 11) and others with the constructive changes going on in the muscles after exercise; but if by constructive changes one means that anabolism for the time being exceeds katabolism in the muscles (which were excessively katabolised during the period of exercise), it is difficult to see why this phase of metabolism should produce an increased formation of urea unless one is to regard both the anabolism and the katabolism as going on at a higher level than usual.

The reason why the skin excretes more urea than usual during exercise is probably because it and the lungs are for the time being the naturally predominating organs of elimination. The most obvious accompaniments of muscular exercise are an increased respiration and perspiration.

During the perspiration of exercise the skin discharges—

(1) More water, which by its evaporation serves to carry off most of the additional heat which is being liberated within the body as the result of the increased oxidations.

(2) More carbonic acid. Schierbeck (12) has shown that as soon as the skin breaks into perspiration its excretion of

carbonic acid is at once increased, and remains high as long as the skin continues to act freely. Hence the skin to some extent relieves the action of the lungs by helping to get rid of the extra carbonic acid in the system during exercise. May not this "respiratory" function of the skin, through the medium of its sweat glands, be the natural explanation of the "sweats" in phthisis and in other lung diseases and in the sweating fevers (malarial, rheumatic, etc.).

(3) More urea, which is determined towards those organs of excretion which are predominantly active at the time, and so is discharged in relatively larger quantity by the sweat glands, and in relatively less amount by the kidneys.

The above holds good when the skin perspires freely from whatever cause; there is an additional discharge of water, carbonic acid, and urea from the skin whenever it breaks into perspiration and so long as it continues to act freely. In confirmation of the increase of cutaneous urea during sweating, a medical confrère tells me that once while he was voyaging in the West Indies his ship was caught in a hurricane which lasted two days and two nights, during which time he and the other passengers were shut down below. Owing to the heat and closeness he and the friend who shared his cabin perspired freely, and both of them had to discard their handkerchiefs and shirts on account of the unpleasant ammoniacal odour which they acquired and which probably was due to a decomposition of the cutaneous urea into ammonium carbonate. Another medical confrère tells me that on the west coast of Africa, in the tropical zone, the skin readily after sweating acquires an ammoniacal odour unless it be frequently cleansed.

Objection may be taken to some of the results stated in this paper, on the ground (1) that the experiments were made on only one subject, namely myself, and (2) that perspiration is a function which varies in different individuals according to personal idiosyncrasy. While admitting that the latter is the case, I would say in regard to the former objection that, owing to the difficulties attendant upon the investigation of the perspiration in a person other than oneself, much greater accuracy is obtained by having this function under constant daily observation in oneself; and again, the necessity and inconvenience of taking a rigidly fixed diet day

after day as was entailed by the several experiments—in one experiment for nearly three weeks—can only be put up with by one who is willing to do it in the interests of scientific medicine, and they are self-imposed conditions which are more satisfactorily carried out by oneself than by another party.

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CLINICAL LECTURE

ON A CASE OF TRAUMATISM OF PART OF THE CAUDA EQUINA, COMPLICATED BY ALCOHOLIC NEURITIS

Delivered to the Class of Clinical Medicine in the University of Edinburgh

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THE case which we have to consider to-day is one that does not come under observation very frequently, and is of great interest as showing the necessity of making a careful examination of the distribution of anæsthesia in cases of nerve injury.

The patient is an upholsterer, aged twenty-seven. In the month of February last, while in New York, he was carrying downstairs on his shoulder three bars, the sides of an iron bedstead, when his foot slipped and he fell. He bumped down a

succession of some nine steps, striking each step with his left buttock. At the same time the iron bars fell from his shoulder and dealt him a severe blow across the front of his thigh. The immediate result was that he suffered some acute pain in the part injured, but he was able to get up and do some more work that day. Later on the same day he felt his left foot rather weak. As this weakness increased, he had to desist from working for two days. In three weeks' time his left lower extremity gradually became completely paralysed from foot to hip, and during the same time he felt a severe pain in his leg and thigh, which extended from above downwards. The limb became ultimately, according to the account given, practically anæsthetic. The effect of the injury thus reached its acme in about three weeks. He is perfectly certain that at that time the right leg was entirely unaffected, both as regards motion and sensation. Also, at no time during his illness, and this is an important point, has his bladder or rectum been at all affected. He has always had perfect control over the functions of these organs. These two facts are of the utmost importance for the diagnosis, as we shall see immediately. The patient remained much in the same condition until about the middle of August. New symptoms then developed. He began to lose power in his right foot, and now he cannot flex the right ankle, and has considerable pain in the calf of same leg—the condition of the left leg remaining nearly in *statu quo.*

We have now to consider to what lesion exactly this condition is due.

(1) When a person falls from a height upon the lower end of the spinal column, paralysis of the lower part of the body and limbs is very apt to occur—paralysis both of motion and sensation, more or less localised. This results, in most cases, either from hæmorrhage into the substance of the spinal cord, or from hæmorrhage inside the dura mater, compressing the spinal cord and nerve roots. These are the commonest causes. Another is that there may be a fracture or dislocation of some of the vertebræ. In the case of subdural hæmorrhage, the blood tends to accumulate within the funnel-shaped lower end of the dura mater, and to extend from below upwards. In such a case there is practically always an interference, due to compression, with the functions of the lower end of the cord

—that is, with the functions of the rectum and bladder, supplied by the third, fourth, and fifth sacral nerves; while there is also a sensory impairment of a very remarkable distribution. The cutaneous surface supplied by these three nerves forms a saddle-shaped area on the buttocks, extending also a short distance down the back of the thigh. This area is marked S_3 and S_4 in Plate VII. These nerves also supply the skin of the penis and scrotum. If the hæmorrhage extends further up the cord, as it may in some severe injuries, so will the paralysis extend further, from the feet upwards, according to the amount of the hæmorrhage and interference with the nerve roots; and as the effused blood tends to surround the cord, the lesion will also always be bilateral. You will remember that in this case the lesion was, in the first instance (for several months) unilateral. Therefore, for this reason, and also because the functions of bladder and rectum were spared, we may exclude the possibility of hæmorrhage into the dura mater.

(2) In the case of dislocation of a vertebra, the pressure is also, almost invariably, bilateral, but one can understand that there might be a partial fracture of a lamina, and that a spicule of bone may be so driven inwards as to injure one side of the cord alone.

In such a case it might be expected *a priori* that motion and sensation would be paralysed on the same side. The actual result of such a unilateral lesion is, however, very different from this, as was first demonstrated by the French physiologist, Brown-Sequard, and is generally spoken of as the Brown-Sequard symptom. There is found in such a case (Fig. 1)—

On the Side of the Lesion.	On the Opposite Side.
a. Below its level.	
A. Motor paralysis, with increase of knee jerk, and usually ankle clonus. Impaired muscular sense. Hyperæsthesia. Elevation of temperature.	B. Anæsthesia to all forms of sensibility, *i.e.* touch, pain and temperature.
b. At its level.	
C. Anæsthesia. Atrophic muscular paralysis.	D. Hyperæsthesia.

This is illustrated graphically in the accompanying diagram, which is supposed to indicate the result of a lesion to the left side of the cord.

You will see that the case before us cannot be explained by a unilateral injury to the cord, for without going into the finer details of the symptoms, we see at once that sensation and motion were both paralysed on the side of the injury.

You observe that on the left side there is absolute loss of power of moving any muscle (with the exception of a slight voluntary contraction of the calf-muscles which has appeared during the last few days); also that there is no loss of sensation on the right side, but that the anæsthesia which is present is entirely in the left limb, *i.e.* in the same side as the lesion.

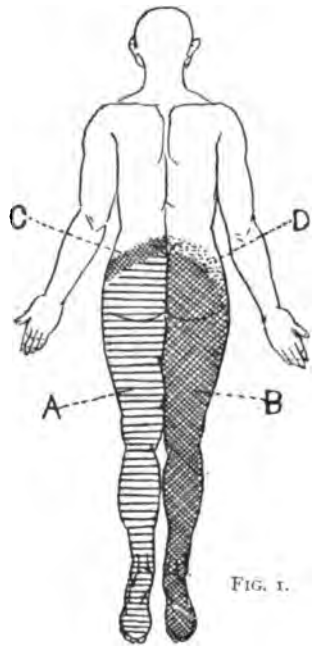


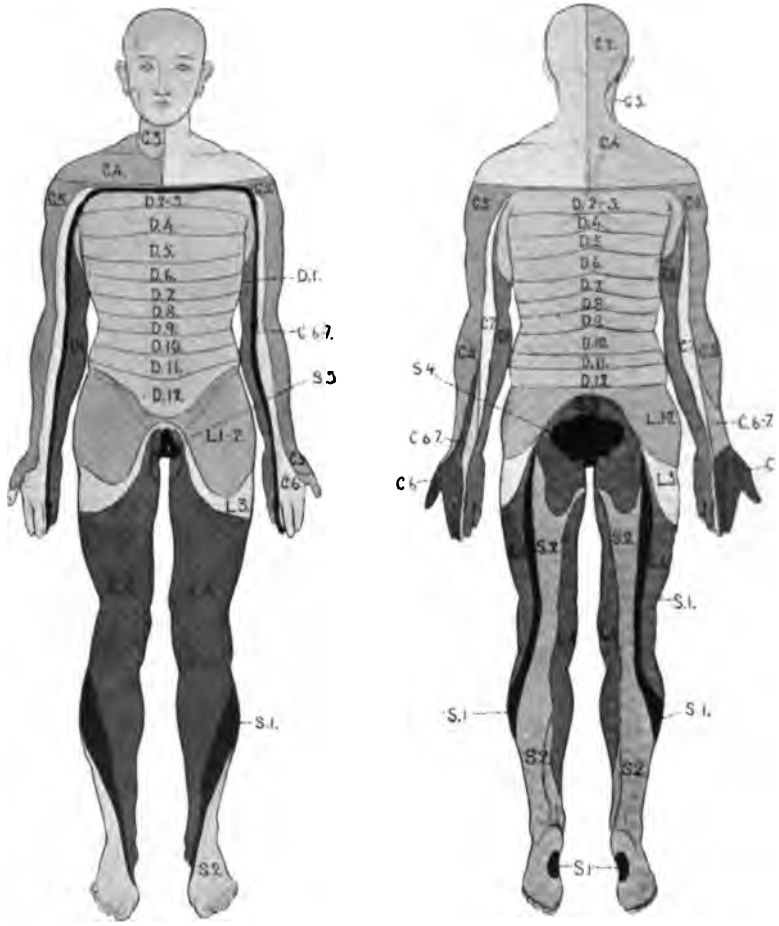
FIG. 1.

We have, therefore, put out of account hæmorrhage, and a unilateral lesion of the spinal cord.

(3) Let us consider, further, another possible cause. The patient fell upon his buttock, and his thigh was hit in front by the iron bars. Such an accident might have directly injured nerve trunks. Both his great sciatic and anterior crural might have been injured. There might have been direct destruction of some of the nerve fibres; and super-added to that, there might have been an inflammatory condition of the nerve. That is to say, the nerve might have been locally congested, infiltrated with leucocytes, and subsequently compressed by the formation of fibrous tissue. This is a condition which might have spread up and down the nerve. My first impression was that this was the cause of the paralysis. I was rather puzzled, however, to account for the loss of power in the other leg. It is, of course, a pathological fact that neuritis or a process of inflammation, when it once starts, may spread up or down a nerve along its lymphatic sheath; and it is perfectly easy to understand that

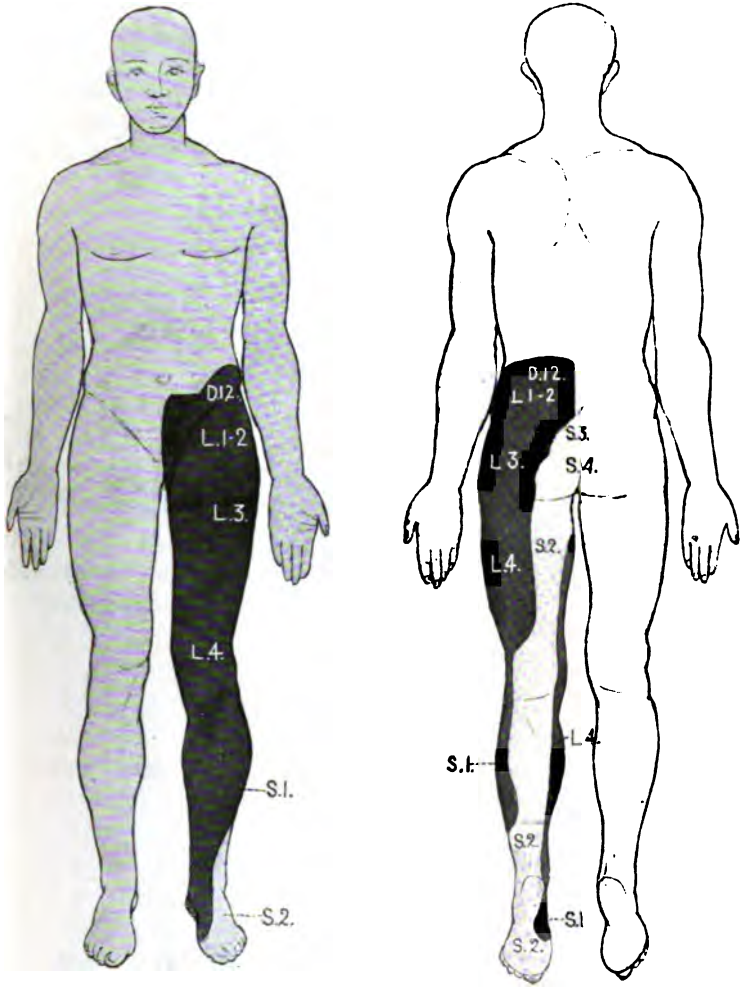
it could proceed up to the spinal cord, but not so easy to see how it could pass across it, and go down the other side. That explanation, therefore, did not satisfy me. The solution of the question was obtained by a more careful examination of the anæsthesia than had been made at first. If the inflammation had spread up and down the course of a particular nerve—say the sciatic or anterior crural, the area of anæsthesia would have corresponded to the distribution of that nerve. But what do we find? The limb was not completely anæsthetic. The greater part of the foot, the outer part of the front of the leg, a narrow strip along the back of the thigh, and an area extending, in the buttock, half way from the middle line to its outer periphery, the penis and scrotum had escaped, while a large area, delineated black in the accompanying figure, was completely anæsthetic.

These areas do not correspond to the cutaneous distribution of any group of sensory nerves. If we compare the effects of this man's lesion with the cutaneous representation of the various nerve roots, which has been very carefully worked out by Thorburn, and more recently by Kocher of Berne (Plate VII.), we find that the area supplied by the first sacral segment is a strip running down the outer side of the calf to the sole of the foot. The area of anæsthesia on the patient's foot corresponds to the first sacral segment, and the remainder of the anæsthetic area corresponds fairly closely to the distribution of the twelfth dorsal, first, second, third, fourth, and fifth lumbar segments. We notice further that the skin surface corresponding to the third, fourth, and fifth sacral segments are perfectly sensitive. The injury here, therefore, affects the twelfth dorsal, all the lumbar, and first sacral segments, and has left untouched those below. We have seen that the lesion is not in the cord; and it is not in the peripheral distribution of any nerve; we are therefore driven to the conclusion that the lesion must affect the above-mentioned nerve roots. Can this man have had such an injury to his spinal column as would affect the twelfth dorsal, all the lumbar, and first sacral nerves, at their point of outlet from the foramina? It is not conceivable that such an extensive injury could have happened without any external evidence. The vertebral spines are not tender, nor is there any displacement. In all probability, therefore, the injury must have been a small one, and one which must



SENSORY ROOT AREAS (AFTER KOCHER).

PLATE VII.—TO ILLUSTRATE DR BRUCE'S PAPER.



DR BRUCE'S CASE—ANÆSTHETIC AREAS BLACK.

PLATE VIII.—TO ILLUSTRATE DR BRUCE'S PAPER.

have developed in the course of three weeks. Now there is a possibility of such an injury occurring in the cauda equina—the roots here pass downwards very close together, and there is a possibility of an injury pressing on the affected roots without affecting the cord itself, and also sparing the lower roots. On comparing the relationships of the roots to the vertebra, I find that a lesion at the level of the twelfth dorsal vertebra would exactly account for the extent of the anæsthesia. We therefore suppose that some form of pressure has been exerted, either by the vertebra or from injury to the dura mater, or hæmorrhage between the vertebra and the dura mater pressing upon the nerve roots at this point.

But how about the loss of power in the other foot, which took place in August, and which was such a puzzling feature in the case? We obtained a clue to the cause of this from the consideration of two facts. You will remember that the patient had no anæsthesia on the right side; and you will observe that if I pinch his calf or thigh on the right side, however lightly, he has exquisite pain. Instead of want of sensation as in the left leg, there is hyperæsthesia. Further, on pressing the muscles in sensitive areas of the left leg there is similar hyperæsthesia. This condition is, I think, interpreted by the history before coming into the hospital, and is, in my opinion, due to a cause independent of his injury. For two months after leaving the hospital in New York, and before coming in to our infirmary, he had been drinking exceedingly heavily. The result has been to induce an alcoholic neuritis, complicating the former injury. One of the features characteristic of alcoholic neuritis, though found also in other forms, is a peculiar symmetrical hyperæsthesia on pressure on the muscles. This gives us the clue to the paralysis of the right foot. It is simply a further consequence of the alcoholic neuritis. The effect of the alcohol on the left limb was masked by the previous injury to the nerves, which had already caused a paralysis of the foot.

In conclusion, I wish to point out to you that it is to the examination of the sensory system that we owe the diagnosis of the position of the lesion causing the paralysis in the left leg, and that it is to the condition of muscular sensibility that we owe the knowledge of the cause of the paralysis of the right foot.

Therefore, I would again impress upon you the necessity, in all obscure cases, of not omitting the careful examination of the cutaneous and muscular sensibility.

Meetings of Societies

The Edinburgh Medico-Chirurgical Society

THE fourth meeting of the session was held on Wednesday, 17th January, Mr A. G. Miller, the President, in the chair.

Dr Norman Walker showed a case of acne varioliformis. Patient was a man on whose forehead there had been an eruption for two years; the scarring was like that of small-pox and there were no necrotic areas. There was no trace of specific lesion discoverable, but the condition had somewhat improved under iodide.

Dr C. W. MacGillivray showed a patient after fracture of the skull.

Dr Charles Begg read a paper on "Clinical experience with salol in the treatment of small-pox," which will appear in a future number of *The Scottish Medical and Surgical Journal.*

In the course of the remarks which followed, Dr Norman Walker pointed out that the salicyl compounds were of much more service in various irritable skin diseases than arsenic, for the salicylic acid, being excreted in the sweat, was carried to the exact point at which it was wanted.

Dr Harvey Littlejohn and Mr Miller also made remarks.

Mr Shaw M'Laren read a paper on "Permanent dislocation of the patella."

The dislocation is a displacement of the patella outwards, so that it lies, even in extension of the knee, in most cases, to the outside of the middle line, and in flexion it slips outwards and backwards till it lies on the outer surface of the external condyle of the femur, its anterior surface directed partly outwards; the patient's gait is most insecure, and frequent falls and synovitis of the knee are the result.

In most cases a fall and consequent injury to the knee first direct attention to the condition, and indeed may increase the dislocation to a certain extent; but it really depends on a congenital defect, viz., a deformity of the external condyle and trochlea of the femur. In most of the cases recorded one or other of the following conditions will be found:—(1) the dislocation, or some accompanying deformity actually noticed at birth; (2) the late age at which the child learnt to walk, or else insecurity in walking during childhood; (3) rachitis; (4) genu valgum; (5) outward rotation of the leg; (6) ill-developed

patella; (7) abnormality in the shape of the external condyle and patellar fossa of the femur.

The quadriceps in passing from its origin to its insertion forms an obtuse angle at the point of which the patella lies. In action it would pull the patella outwards till it came to be in a straight line between the origin and insertion of the muscle, were this not prevented by the obstacle which the prominence of the external condyle presents. The ridge of the external condyle keeps the patella in the middle line. Should the fossa patellæ be too shallow, or the ridge of the external condyle not be prominent enough, the patella will be dangerously far out on the trochlea—a case of slight dislocation. It then only needs a severe contraction of the muscle or a slight trauma to shoot the patella off the front of the condyle altogether, and bring it to lie on the outside of the femur. Or in cases in which the condyle is markedly deficient, the complete dislocation may exist from birth or the first years of life. In any case the structures on the outer sides of the patella eventually contract and prevent the possibility of the bone retaining its proper position.

As regards treatment various methods have been tried. Roux, in a traumatic case, divided the vastus externus, detached the ligamentum patellæ, and nailed it further inwards to the tibia. Goldthwait, in a bilateral case, adopted a similar procedure in treating the one knee, while, on the other side, he detached the tubercle of the tibia and nailed it further inwards; he gives the preference to the latter method. On both sides he divided the outer part of the capsule to allow the patella to be drawn inwards. The result was good. Lucas Championnière performed a more severe operation. He opened the joint, cut a groove on the surface of the femur, and fixed the patella in it by sutures. Bilton Pollard followed this idea. He corrected the genu valgum by osteotomy, but the patient's walk was no more secure. To correct the dislocation, he made first an external incision through which he divided the insertion of the vastus externus and the outer part of the capsule; through an internal incision he chiselled out a groove in the cartilage and bone of the femur. The patella placed in this slid up and down normally in flexion and extension of the knee. Lastly, he excised a piece of the capsule on its inner side, and braced up the joint. After a month the patient walked well.

Mr Shaw M'Laren's operation consisted in liberating the patella by dividing on the outside whatever contracted structures prevented it assuming its normal position, and then mooring it there by sutures passed through its inner border. The result was satisfactory. Though this method seems rational and effective, it is possible the more severe procedure of opening the joint and enlarging the groove for the patella

might in some cases be necessary; while, again, transplanting the ligamentum patellæ inwards should be reserved for cases where the outward rotation of the leg exists to a high degree.

Remarks were made by Dr MacGillivray and Mr Caird.

Dr Harvey Littlejohn read a paper "on a case of murder and suicide by cut-throat."

Edinburgh Obstetrical Society

THE third ordinary meeting was held on January 10th, Dr Milne Murray, President, in the chair. Professor Simpson showed specimens from a case of Cæsarian section. A case of that very rare condition—*abdominal teratoma or fœtus in fœto*—was exhibited by Dr Ballantyne.

Dr James Ritchie read *notes of a case of albuminuria, with multiple pregnancy, papyraceous fœtus and placenta prævia.* The patient, æt. 32 years, had one previous child aged three years. She menstruated last on February 28th, 1899. On July 22nd he found her suffering from severe headache, shortness of breath and debility, the face, feet, legs and thighs were much swollen, the urine scanty and containing much albumen, but no blood or tube casts. Under rest, aperients, mustard to nape of neck, sodium bromide, and very light diet, the symptoms disappeared almost entirely in two days. On September 20th she had slight hæmorrhage followed next day by a sharp bleeding. The os was partially dilated, and projecting from it was a triangular piece of placenta. As no further hæmorrhage occurred the case was left to nature. Two hours sufficed to produce a macerated fœtus of about 5½ months' growth, the placenta following quickly. Next day another portion of placenta came away and shreds continued to come for some days. In a few days the albumen quite disappeared from the urine.

The placenta on examination was found to consist of one mass, but had not the same consistency throughout, there being a small hard portion and a larger of more normal appearance. The membranes consisted of a single chorion and two amniotic sacs, the smaller of which, lying near the os, contained a flattened or papyraceous fœtus. The portion of placenta belonging to this fœtus had become functionless and exsanguine, and was that felt presenting. Though for seven weeks before confinement the quantity of albumen was small — under 0·25 by Esbach's method — the nutrition of the fœtus was seriously interfered with. This is another example of what has often been observed, viz., that albuminuria of pregnancy, though slight, seriously affects the nutrition of the child. Prof. Davis of Philadelphia says that serum albumen in the urine is only of

importance when in excess and when accompanied by kidney débris. Neither of these statements is in accord with Dr Ritchie's experience.

The cause of albuminuria is practically unknown. Meyer, in Copenhagen, found it in 5·4 per cent. of pregnant women, and during the last thirty days of pregnancy, only 28-29 per cent. were free. During pregnancy, apart from functional albuminurias and those due to Bright's or heart disease, there are some which are due to pregnancy. In these the albumen occurs not only during pregnancy but as a consequence of it, and its occurrence during one pregnancy does not indicate that it will recur. This albuminuria is due to some condition peculiar to pregnancy —probably some unusual condition in the relation of the products of conception to the mother, and associated with the development of a toxic condition of the blood. Although a micro-organism has been described in connection with eclampsia —a planococcus—it has not yet been proved to have a causal relation. The cause of albuminuria of pregnancy is peculiarly prejudicial to the nutrition of the fœtus, and the severity of the result is not in proportion to the amount of the albumen. Our knowledge of the pathology of the albuminuria of pregnancy is so defective as to provide few indications for treatment, yet there is possible grave danger to both mother and fœtus. The following principles seem to be indicated:—(1) reduce the amount of nitrogenous food; (2) limit the production of toxic material; (3) favour elimination. Rest in bed on a milk diet, with attention to the bowels and kidneys, fulfil these. Digitalis as a diuretic should not be used, as it increases the already too great tension of the pulse. How long is this treatment to be kept up? Dr Ritchie thinks that if the dropsy be gone and the urine copious with but little albumen, some liberties may be taken. The paper was freely discussed by the Fellows. Dr Ballantyne agreed with Valois in saying that for the sake of the fœtus the treatment should be continued for some time after the disappearance of the albumen.

Dr Berry Hart then read a most interesting paper setting forth an entirely new and original theory on *the cause of the differentiation of connective tissue in the human fœtus, with reference to the essential nature of the hydatid mole.* The following is a short summary of the paper, which will shortly be published *in extenso.*

Dr Hart first discussed the development of connective tissue, especially in the myxomatous stage, and on to the fully developed type. He pointed out that the myxomatous stage was permanent for the umbilical cord, that the tissue in the villi fell short of fully developed connective tissue, while that in the rest

of the fœtus went on ultimately to the higher form. He believed that the relations of the thyroid to myxœdema in the adult, as well as the structure and the evidence of activity of the thyroid in the fœtus, made it highly probable that F. Semon's theory was applicable in fœtal life; that, in fact, the thyroid secretion favoured the development of connective tissues from the myxomatous to the higher type. He pointed out that the umbilical cord was not varcularised, that the thyroid secretion did not affect it, and thus it remained of a mucous type. He applied the same reasoning to explain the condition of the connective tissue in the villi and in the vitreous humour.

He summed up as follows:—

1. There is anatomical and clinical, but not as yet experimental evidence, that the thyroid is functionally active in the human embryo.

2. It begins to develop in fœtuses of four millimetres, *i.e.* about the third week.

3. It probably causes the development of connective tissue to pass from the myxomatous to a higher stage, thus, (1) the cord without vessels and presumably without thyroidisation remains myxomatous; (2) the adult vitreous under the same conditions remains allied to myxomatous tissue; (3) tissues like the villi are also more or less myxomatous according to their age and length of exposure to vascularisation.

4. The free covering of the somatopleure is also probably affected by the thyroid.

5. The hydatid mole may be regarded as acute myxœdema of the chorion.

6. Thyroid extract should be administered after hydatid mole abortion.

In the hydatid mole we have a mass of tissue of a myxomatous type, with, in almost every case, no fœtus. He believed, therefore, from the evidence he had brought forward, that this view of the myxœdematous nature of the chorion was a highly probable one.

Dr Hart then gave a demonstration on the early changes at the caudal end of the primitive gut in the human embryo, and its bearing on a case of persistent end gut in an infant, and on extroversion of the bladder.

Glasgow Medico-Chirurgical Society

AT a meeting on January 12th, Dr J. Barr Stevens read notes of a case of superficial emphysema occurring during labour. The patient was a primipara, in normal labour, with very strong pains, and but little advance of the head. A slight cracking

noise was heard, and shortly after there was noticed emphysema of the face, shoulders, etc. The delivery was completed by forceps. There was no fracture of ribs, or pain, or difficulty in breathing, or pneumothorax, or hæmoptisis. The puerperium was normal, and the temperature. There was no injury of the larynx. The emphysema disappeared by the seventh day. Dr Stevens quoted remarks by the late Professor Coats on the occurrence of superficial emphysema in whooping-cough and diphtheria under conditions of great expiratory force, with obstruction in the air passages. In his case, Dr Stevens had concluded that one or more pulmonary vesicles had ruptured. Of these cases about thirty had been described, the first one in 1874. There was usually the association of violent labour pains and rigidity of the passages. Dr Stevens gave a resumé of the literature on the subject, referring to numerous cases.

Dr J. K. Kelly thanked Dr Stevens for his most interesting paper, and referred to his own experience of one case, in which he had concluded that a rupture had taken place in the mucous membrane of the larynx.

Dr Munro Kerr had not seen a case, but had recently read an article on the subject by a German author, and it was there noted that such patients were usually primiparous, with difficulty in the labour.

There had usually been difficulty in breathing before the emphysema was noticed. In one case a fatal result was recorded. The emphysema usually disappeared by the seventh or eighth day. The case recorded by Dr Stevens was a typical one.

Dr Watson had seen one case, and had considered that the air had come by a lesion in the upper respiratory tract.

Dr Edgar read a paper on Post-Climacteric Hæmorrhage. A variety of cases were related, including cancer of cervix, cancer of corpus uteri, mucous polypus of corpus uteri, fibroid of uterus, and chronic ovarian and pelvic inflammatory disease. Great stress was laid on the importance of suspecting, and making an early diagnosis of, malignant disease, in many cases of this class.

Dr Marshall, in reference to a remark of Dr Edgar's on the early age at which cancer of the uterus might be met with, said he had found it in patients of 33, 32, and 28 years of age. It was a great mistake, as Dr Edgar had pointed out, to suppose that cancer of the uterus occurred only at or about the menopause. With regard to post-climacteric hæmorrhage, he had found it caused by endocervicitis, with extensive erosion of the cervix, and by mucous polypus of the cervix (two cases).

Dr Kelly had often preached that cancer of the uterus must

be looked for at the climacteric period, though of course it was often found at a much earlier age. He also emphasised the fact that cancer of the uterus was much more often found in nulliparæ than some people suspected.

Dr J. K. Kelly read notes on some gynæcological cases, and demonstrated a number of interesting specimens.

These included a hydrosalpinx impacted behind the uterus, with thrombosis of the ovary. Gangrene of the ovary was taking place. There had been symptoms similar to those of a ruptured tubal pregnancy. The operation had been successful, and a pregnancy, present at the time, had not been interrupted.

The next two specimens were of pyosalpinx, in which the radical operation per vaginam had been performed.

Three specimens of pyosalpinx were shown, which had been removed by abdominal section.

The question of vaginal *versus* abdominal operation in these cases was in an acute stage. The radical extirpation, per vaginam, was satisfactory, but very difficult. It was said by some to be safer than the abdominal operation, but Dr Kelly inclined to the latter. In some of the cases the symptoms were like those of a ruptured tubal pregnancy.

A parovarian cyst was shown which had been removed with the tube and ovary. From the excessive pain present in this case, before operation, it had been thought to be a pyosalpinx, or other inflammatory lesion.

Then followed a case of extra-uterine pregnancy, showing the embryo in the midst of a blood-clot; a case of myoma of the uterus, very large; and a case of myoma with pregnant uterus, the whole having been successfully removed, and the patient leaving hospital, for further care at home, in seventeen days.

Also a case of cancer of uterus removed by vaginal hysterectomy, and a case of cancer of the cervix, with myomata at the sides of the uterus.

Forfarshire Medical Association

A CLINICAL evening of the Association was held in University College, Dundee, on January 10th, Dr Templeman, the Vice-President, in the chair.

Professor Kynoch showed a child from whom he had removed a renal sarcoma three years before by laparo-nephrectomy. Six other cases of laparo-nephrectomy on children under two years of age had been reported as alive and well three years after operation. There was still considerable difference of

opinion in regard to the pathology of these renal tumours. Although only five per cent. of all reported cases had recovered, the chance of a cure ought to be offered.

Drs Buist and M'Vicar also spoke.

Dr M'Gillivray showed (1) boy with rupture of the choroid, (2) patient who after injury had suffered from a dilatation of one pupil which he was inclined to consider as due to irritation—an irritation mydriasis.

Dr M'Vicar showed a patient with a large cystic swelling, which extended from the anterior border of the sterno-mastoid to the middle line, with branching processes under the same muscle and up into the floor of the mouth. The fluid was viscid and clear, and when it escaped by the mouth had a sweetish taste. It appeared to be a branchial cyst and was of interest, as such cases had been known to assume an epitheliomatous character.

Dr Gray showed for Professor M'Ewan specimens of (1) fracture, base of skull; (2) epithelioma, tongue; (3) limb from a case of senile gangrene.

Dr Gray showed a cast of an arm with lipomata.

Professor Kynoch illustrated by a painting an unusual pigmentation of the central linear cicatrix as well as the stitch scars of a case of ovariotomy he had performed during pregnancy. A scar of two years' duration on the breast had remained unaffected. He asked whether the condition was due to the ovariotomy or to the delicate skin, and in relation to this mentioned that Bloxall had reported a case in which the scar left after removal of a dermoid, had become pigmented during a subsequent pregnancy. Dr Buist had seen a like scar pigmentation during a pregnancy following immediately after the removal of one ovary.

Professor Kynoch showed a multilocular ovarian cyst, and remarked on the few adhesions present notwithstanding the fact that part of the cyst had been ruptured by an accident some time previously.

Professor Marshall and Dr Buist added some comments.

Dr M'Vicar showed (1) an ovarian cyst which in view of the clinical history and the naked eye and microscopic appearance seemed to be an abscess of the ovary, (2) a vermiform appendix with the lumen practically occluded.

Drs Buist and Kynoch discussed some of the points raised.

New Books and New Editions

Practical Diagnosis: the use of Symptoms in the Diagnosis of Disease. By HOBART AMORY HARE, M.D., B.Sc., Professor of Therapeutics in the Jefferson Medical College of Philadelphia. Fourth Edition. 8vo, pp. 631, illustrated with 205 engravings and 14 coloured plates. London: Henry Kimpton. Price 21s. net.

WHEN a book reaches its fourth edition in a little over three years it may be safely assumed that it meets the requirements of a large section of the profession. As this general proposition may be taken as accurate, Dr Hare is to be congratulated on the marked success of his work. As was said when an early edition of this book was noticed in these pages, the plan of the work is the reverse of that usually followed in handbooks of medical diagnosis. We may again mention that, as the title indicates, the book treats of the use of symptoms in the diagnosis of disease; that is to say, disorder and disease are sought to be approached through the medium of a leading or prominent symptom. Although this starting-point is not that of the systematic treatise, it is to be recognised that Dr Hare's method is the way of practice of the physician. It is even more, for it is the standpoint from which clinical medicine started, and it is the accumulated experience and observation of generations of capable men that made the systematic treatise possible and have led to the standard of excellence attained by recent works. To the present-day schoolman the method does not appeal and is not readily approved of; still it has a practical value, and the book doubtless has been, and will continue to be, of much service. Knowledge has been accumulated in such detail that no one method of communicating it can be perfect and free from criticism, and this is true of the work before us. The first part is devoted to "the manifestation of disease in organs," and includes chapters on the face and head, the hands and arms, and the feet and legs; and in these are described and figured with great clearness spasms, tremors, paraplegia, etc.; but then comes a separate chapter on hemiplegia. Chapter nine deals with

the abdomen and abdominal viscera, and chapter twelve with the bowels and fæces. The second part is devoted to "the manifestation of disease by symptoms," and there are chapters on headache and vertigo, pain, cough, and expectoration, tendon-reflexes and muscle-tone, and on speech. There is no chapter on anæmia in this section; it is considered in the chapter on blood in the first part. From this it will be seen that there are difficulties in the way of realising fully any idea which deviates from that which is more generally followed. Dr Hare has probably made more of this method than anyone else could have realised, for the book is evidently the work of an acute observer and an able physician of large and varied experience, qualities which cannot but make themselves felt when a work of this kind is undertaken. The book can be cordially commended as a *first aid* to the practitioner and also to the senior student. The work is very freely illustrated by original drawings and drawings from other sources, and this adds much to the attractiveness of an admirably got up book.

Raynaud's Disease: Its History, Causes, Symptoms, Morbid Relations, Pathology, and Treatment. By THOMAS KIRKPATRICK MUNRO, M.A., M.D. Crown 8vo, pp. 250. Glasgow: James Maclehose & Sons. 1899.

WE are indebted to Dr Munro, an ardent admirer of Raynaud, for a very full account of the causes, symptoms, pathology, and treatment of the condition first fully described by Raynaud as "local asphyxia and symmetrical gangrene of the extremities." Though there is much still awanting to our accurate knowledge of the etiology and pathology of the condition, Dr Munro deserves full acknowledgment for the good service he has done in bringing together and systematising the literature of the subject. If one were disposed to criticise, it might be advanced that there is perhaps too much of the bibliographer and too little of the author—too little of the "number and character of the cases" which have come under the author's notice. Considerable space is devoted to the subjects of morbid relations and pathology. The author rather inclines to accept the theory of an increased excitability of the vaso-motor centres

in the cerebro-spinal axis, Raynaud's original theory, though he quite appreciates the many difficulties which beset the acceptance of the thesis. Definite neuritis he finds has been frequently absent, and when present in typical cases is capable of being explained as a secondary phenomenon and not a casual factor.

The monograph is a careful and painstaking piece of work, and is well worth the perusal of those interested in the subject.

A Manual of Surgery for Students and Practitioners. By WILLIAM ROSE, M.B., B.S.Lond., F.R.C.S., Professor of Clinical Surgery, King's College, London, Senior Surgeon to King's College Hospital, etc.; and ALBERT CARLESS, M.S.Lond., F.R.C.S., Surgeon to King's College Hospital, and Teacher of Operative Surgery in King's College, London, etc. Second Edition. Demy 8vo, pp. 1190, 400 illustrations. University Series. London: Ballière, Tindal & Cox. Price 21s. net.

THE second edition of this text-book of Surgery has been issued only a year after the first edition, and when we look into it we find no difficulty in accounting for its popularity. The authors are familiar with all recent progress in Surgery, and have exercised a wise discretion in bringing forward only what is really useful. Students are thus not left in doubt as to which of several possible methods is the best—a common fault with text-books. The general arrangement is well planned, and the mode of expression is clear and interesting, neither tiresomely diffuse nor compressed like a cram book. The type is sufficiently large to be read without effort. Dark type and italics are used effectively for headings and for drawing attention to important points. The illustrations are well chosen and useful. Altogether we feel that this text-book has well deserved its success, and we can confidently recommend it to our Scottish students.

Lehrbuch der Nervenkrankheiten (Text-book of Nervous Diseases). By PROFESSOR OPPENHEIM. 2nd Edition. Lex. 8vo, pp. 985. 287 illustrations. Berlin: S. Karger, 1898. Price 25s.

IT is not surprising that a second edition of this work should be called for so soon after the appearance of the first. There are many text-books on nervous diseases, but we know none which

surpasses Oppenheim's well-known work in respect of lucidity of expression and wonderful fullness of detail, combined with smallness of bulk; it is essentially a readable book, and the more one uses it the more one is surprised at the number of facts which are packed into such small space.

In general scope and arrangement, the second edition follows the first very closely, but each chapter has been thoroughly revised and brought up to date, and new chapters have been added. The number of illustrations has been considerably increased, so that the wealth of excellent photographs, illustrating the clinical phenomena of the various diseases, is now one of the best features of the work.

If one part is better than another, we should perhaps single out for special praise the first or general part, which is devoted to the method of examination and general symptomatology of nervous diseases: the description of the difficulties in the way of examination, and of the methods of overcoming them is excellent.

The special description of the diseases of the various parts of the nervous system—spinal cord, peripheral nerves, brain and sympathetic system—is preceded in each case by a succinct but wonderfully complete account of the recent enormous advances made in our knowledge of the anatomy and physiology of the nervous system by the methods of Golgi, Marchi, Nissl, etc., etc. We would draw special attention to the summary of facts known regarding the physiology of the cord (p. 102) and regarding "Retrograde Degeneration" (pp. 110, 111).

Oppenheim includes peripheral palsies of the cranial nerves in the section dealing with diseases of the peripheral nerves: although this may be anatomically correct, it would, we think, be better to place them in the part which treats of diseases of the brain, mainly because a knowledge of the anatomical relations of the nerves at the base of brain contributes so much to the understanding of the peripheral affections and, hence, the excellent chapters dealing with the anatomy and physiology of the brain (pp. 448-488) might well precede the systematic description of the peripheral palsies of the nerves. Further, it would be of advantage to have all the forms of affection of any cranial nerve, central and peripheral, considered together, especially as it is not always possible to draw a sharp line between peripheral and central affection.

The author expressly states in the preface to the first edition that the work is mainly a clinical one, and that, therefore, the greater part of the space is devoted to symptomatology, diagnosis, prognosis and treatment, while only the essentials of the pathological anatomy are given: we confess to a feeling of

disappointment that he has not found it possible, in the second edition, to extend the scope of the work as far as pathology is concerned. We should have liked, for example, to have had his views given much more fully regarding the pathology of myelitis or the pathogenesis of syringomyelia.

The number of authors, whose names are referred to, is much greater than in the first edition: this is, in our opinion, a distinct improvement, and we hope that in later editions the list will still further be extended, and that references to literature will be added. We readily recognise that the absence of these and, perhaps, also of a fuller index, does not detract from the value of the work as a clinical text-book, but these additions—without materially increasing the bulk of the book—would enhance its great usefulness as a work of reference.

We heartily commend the book to our readers; it should be found in the library of all who take an interest, special or general, in the study of nervous diseases.

Modern Cremation: Its History and Practice to this Present Date, with information relating to all recently improved arrangements made by the Cremation Society of England. By SIR HENRY THOMPSON, Bart., F.R.C.S., M.B. Lond., etc. Third Edition. Small 8vo, pp. vii., 187. London: Smith, Elder & Co. 1899. Price 2s.

THE history of the Cremation movement in England is largely associated with the name of the distinguished author of this brochure, and no one could state more clearly and fairly the argument in favour of the practice. The book is already so well known that it is only necessary to mention the appearance of this new edition, in which new matter has been added and some alterations made in the arrangement of the work. Those who desire reform in the present system of Death Registration, will read with pleasure the pages devoted to the exposition of its inefficiency and of its dangers.

The Schott Method of the Treatment of Chronic Diseases of the Heart, with an account of the Nauheim Baths and of the Therapeutic Exercises. By W. BEZLEY THORNE, M.D., M.R.C.P. Third Edition. 8vo, pp. 132, illustrated. London: J. & A. Churchhill. Price 6s.

DR BEZLY THORNE'S monograph on the Nauheim treatment is already well known to the profession, and the appearance of three editions of it within four years gives an *imprimatur* to

which the critic readily subscribes. The facts are clearly described and judiciously discussed, and there is no attempt to magnify the method beyond its real limits. In this new edition there has been a considerable rearrangement of matter and two new chapters have been added.

Scientia. Biologie No. 3, Les Fonctions rénales. Par FRENKEL, Professeur aggrégé à la Faculté de Médecine de Toulouse. Pp. 84. Carré et C. Naud.

FRENKEL in this brochure gives a fair summary of the present position of the various questions settled and unsettled regarding the function of the kidneys. He is mostly positive on the most modern doctrines, such as renal insufficiency, the internal secretion of the kidneys, and urinary toxicity, but the limits of his paper do not admit of any exhaustive discussion. The whole subject is clearly and well summarised.

The Edinburgh University Students' Handbook. No. IV., 1899-1900. 18mo, pp. 108. Price 4d.

THE Students' Handbook, published by the Students' Representative Council, is a little volume replete with useful information for the student, ranging from a historical sketch of the University to the E. U. Women's Cycling Club. At the modest price of fourpence, it cannot be too heartily recommended to the notice of the students. From the last paragraph of the introduction, we should say that the Handbook Committee show undoubted business instinct which only wants a larger field to even further reduce the price of the volume to the purchaser.

Obituary

George Elder, M.D., F.R.C.P.Ed.

BY the death of Dr George Elder, the Edinburgh Medical School has lost one of the most promising of its younger members. He died of pneumonia on January 6th, after a short illness.

Dr Elder was born in the parish of Carnwath, Lanarkshire, in the year 1869, but passed almost the whole of his early life near Haddington, and was educated at the Knox Institute there. He studied medicine at Edinburgh University between 1887 and 1891. As a student his career was brilliant; he obtained the Vans Dunlop scholarship, and graduated with first-class honours. After graduating he held resident appointments in the Royal Infirmary, in the Royal Maternity Hospital, and in the Royal Hospital for Sick Children. He was also chosen one of the annual presidents of the Royal Medical Society. In 1895 he began practice in Edinburgh. About the same time he became a Fellow of the Royal College of Physicians, and was elected a physician to the New Town Dispensary and assistant physician to Leith Hospital. In 1896 he obtained the degree of M.D. for a thesis on the intracranial circulation, a subject which he was further investigating just before his death. He frequently contributed to the medical magazines, and was, in conjunction with Dr Fowler, author of a recently published book on the *Diseases of Children*.

Dr Elder devoted much time to purely scientific work, yet it was to the bedside rather than to the laboratory that his strongest inclinations turned. There he was—as he desired to be—at his best. He was a man of wide sympathies and interests. Essentially an optimist, he took a sanguine view of life, and enjoyed it to the full. Into the pleasures and duties of social life he entered with the keenest zest. His conversation was vivid and engaging; he delighted in debate. No one was more honest and outspoken, at the same time no one was more kindly and considerate. Insincerity and uncharitableness were equally foreign to his nature. He never paltered with what he believed to be right, but he never formed harsh or hasty judgments; he was never disloyal to his own convictions, but he always dealt gently with the susceptibilities of others. His was a singularly happy disposition. While his talents called forth warm admiration, his cheerfulness, his modesty, his entire lack of affectation, and the charming equability of his temper, won the affection of all who knew him.

Recent Literature

CRITICAL SUMMARIES AND ABSTRACTS

THE STEREOGNOSTIC SENSE

By ASHLEY W. MACKINTOSH, M.A., M.D.,
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COMPARATIVELY little attention has been given by writers in this country to what is generally known as the **Stereognostic Sense**, or, as it may be defined, the power of recognising the nature of an object by *feeling* it or—the hand being the part of the body naturally employed in the act—by *handling* it. Not a few observations bearing on this subject have been recorded within the last few years by writers in other countries, notably America and Germany, and it is the aim of this digest to give a resumé of these records and of the deductions to be drawn therefrom, as it is believed that the subject is one of intrinsic interest and value, and has, moreover, important bearings on wider questions connected with the sensory functions, general and special, of the cerebrum.

Various definitions and terms have been used by different writers for this faculty or its loss. Usually the "stereognostic sense" is spoken of, but "tactile perception" [Sailer (1), Burr (2)], "active touch" [Ziehen, Dana (3)], are also used. By this faculty is meant the recognition of the nature of objects "placed in the hands" [Williamson (4), Dubbers (5), Dana (3)], "by touch" [Burr (2)], "by tactile impressions alone" (Williamson), "by tast-gefühl alone" or "durch betasten" (Dubbers). The loss of the faculty is generally described as "astereognosis," but "touch-paralysis" [Williamson (4)], "tast-lähmung" [Wernicke (6), Dubbers, Oppenheim (7)], "psychic anæsthesia" [Burr (8)], "seelen-anæsthesia" [Oppenheim (7)], "tactile amnesia" [Burr (2)], etc., have also been employed. It is not quite clear that these terms are always used to designate the same thing: in any case, it would be less confusing and more accurate if one, in speaking of the stereognostic sense as defined at the outset, did not use, as synonymous with it, the power of recognition of objects by *touch* or *tactile* impressions, which undoubtedly form an essential part but not the whole of that complex process.

It is generally acknowledged that the stereognostic sense is not a single or simple sense—it depends on a combination of senses. Several factors, probably many, enter into stereognostic perception, and the principal endeavour of many writers has been to determine the relative value of the different factors. It is well, however, to remember from the outset that there is a second and, we believe, a more difficult problem. Recognition

of the nature of an object by handling implies more than simple sensory perception. The various impressions got from "handling" must not only reach the cerebral cortex and be felt in consciousness as separate sensations, they must also be correlated there, and this correlation, either unaided or by the co-operation of associated sensory centres, must lead to recognition by the aid of "memory." In considering the stereognostic sense, we have thus to keep these two problems separate: (1) what part do the various elementary sensations play in the recognition of objects by handling? and (2) how does this complex of sensations lead to the recognition? I shall deal with these in the order named.

Hoffmann (9) studied the subject very fully, and his work forms the basis of our knowledge. His results show that a large number of factors enter into stereognostic perception, and that they are of variable value, although possibly to some extent interchangeable [Dercum (10)]. According to him, the following elements enter into it:—the tactile sense, the sense of localisation of tactile impressions, the space- or distance-sense, the pressure-, muscular-, temperature-, pain-senses, the knowledge of the position of the fingers to each other and to the hand, and the recognition of active and passive movements. He states that the stereognostic sense may be lost, when the tactile-, pain-, temperature-senses, sense of position, appreciation of the weight of bodies, and the power of localisation are preserved. The most important factors are the sense of pressure, the ability to appreciate distance between two points, the recognition of the movements of joints and the recognition of the relative position of the fingers. In not one of his cases, in which any of these senses was preserved, was the stereognostic sense *lost*, although each one may be diminished to a marked degree without the stereognostic sense suffering in a like degree and vice-versa.

Dana (3) believes that in active touch "both the tactile and muscular senses are brought into play."

Williamson (11) says that, while the loss of the stereognostic sense, of course, occurs in all cases with marked anæsthesia, it may be but little, if at all, affected where there is considerable diminution of tactile sensation, or marked inco-ordination (as in Friedreich's disease) or complete motor paralysis. It is "probably an association process dependent on tactile sensation on the power of localising tactile impressions, and on muscular sense, and, sometimes, in addition, on the sense of pressure."

According to Oppenheim (7) (pp. 50, 51), the pressure sense and the senses of passive movement and of position of the limbs are most important for the stereognostic sense: he has frequently seen it implicated where the sense of position alone was

affected; it may be lost, although the senses of touch, pain, and temperature are present.

Sailer (1), who has dealt with the subject in greater detail than any writer since the time of Hoffmann, concludes that "in the case of peripheral disturbance . . . stereognosis is lost if the tactile sense is lost, and it is greatly impaired or lost if the localisation sense is lost; it is usually, but not always, disturbed if the muscular sense is lost, but may persist if any of the other forms of cutaneous sensation are lost or disturbed." His reason for giving more importance to the tactile-localisation sense than to the muscular sense is twofold: (1) he finds that the records of five collected cases (including his own case), with loss or great diminution of the stereognostic sense, show that the localisation sense was lost or greatly impaired in all cases in which record was made of the fact, while the muscular sense was intact in two; (2) by a series of experiments on normal persons, he finds that the size of objects is more accurately told by laying the object on the palm of the hand and fingers—*i.e.* by "contact sense" or a combination of tactile-, pressure- and localisation- sensations—than by placing the object between the thumb and forefinger or between the flexed fingers and palm—*i.e.* by simple muscular sense or knowledge of the exact position of the fingers and hand.

Dercum (10) believes that Sailer is right to attach more importance than Hoffman does to the tactile-localisation sense. He thinks it probable that, in his case of tumour pressing on the medulla and cerebellum, the astereognosis was due to "the partial sensory losses" which interfered with the complete perception of the qualities of the objects handled.

In attempting to draw conclusions as to the relative importance of the various elementary sensations in stereognostic perception, I have tabulated the recorded cases of astereognosis and noted the relative frequency and degree of implication of the various sensations. Strictly speaking, the list should contain only those cases in which the lesion is certainly peripheral. Cases have been recorded in which there was astereognosis with no other sensory disturbance, *i.e.* isolated or pure astereognosis. The only explanation of these seems to be that the lesion was central to the sensory receptive centres and interfered with the higher process by which the sensory impressions lead to the recognition of objects. In any lesion which implicates the cerebral cortex, directly or indirectly, it is probably impossible, especially in view of the great uncertainty as to the location of the sensory centres, to be certain that the resulting astereognosis is not of central origin. In including such cases, therefore, in the list, from which deductions are to be drawn as to the peripheral components of the stereognostic sense, I am conscious that I greatly invalidate the results, and I can only plead lack

of material as my excuse. I have found only one case of astereognosis in which the lesion was certainly not cerebral or even cortical. This is a most interesting case recorded by Dercum (10), in which there was a tumour growing from the occipital bone and pressing on the right side of the medulla and cerebellum. In a series of cases the astereognosis resulted from traumatic lesion of the brain in the region of the central convolutions, often affecting mainly the middle part of the ascending parietal convolution and the adjacent part of the parietal lobe: two such cases have been recorded by Dana (3) (cases 3 and 4) and one by Williams (12), Hudson (13), Riegner (14), Wernicke (6), v. Monakow (15), Dubbers (5), and Burr (8). Cases recorded by Sailer (1) (case 1) and Olmsted (16), although no diagnosis of the site is given, were also due, in all probability, to cerebral lesions. Williamson (11) has put on record five cases with astereognosis: in one (case 2), the lesion was, he thinks, probably a gumma or other syphilitic lesion in the neighbourhood of the right arm-centre or just behind it; two had recovered from hemiplegia due to embolism of a middle cerebral artery; and in the other two the cause was indefinite but probably cerebral thrombosis or embolism. There are thus seventeen cases in all: of these, I have excluded from the list the cases of Williams and Riegner and two cases of Williamson (cases 4 and 5), as the details at my disposal are too incomplete to make them of sufficient value. I have also omitted a third case of Williamson's (case 2), as it evidently comes under the category of cases of isolated astereognosis, there being no loss or impairment of any elementary sense.

The records of the remaining twelve cases with astereognosis show that the condition of the various forms of sensation was as follows :—

	Number of cases in which the various senses were			
	Disturbed			
	Lost	Impaired	Normal	Not mentioned
Touch . . .	0	5	5	2
Pain . . .	0	5	6	1
Temperature . .	0	3	9	0
Localisation . .	1	6	2	3
Pressure . . .	2	2	1	7
Passive Movement .	3	0	2	7
Position . . .	4	1	2	5
Weight . . .	2	2	0	8
"Muscular Sense" .		2		
One or more forms of Muscular Sense	6	3	3	0

Ataxia of the affected arm was present in all cases in which it was tested for, viz., nine cases.

These figures show that:—

(1) In cases of absolute astereognosis, any one of the various forms of sensation has been found normal, with the exception of the muscular sense as tested by the ability to appreciate weights.

(2) The senses of touch, pain and temperature have been more often normal than otherwise, and in no case has any one of these senses been lost.

(3) The localisation sense, where noted, has been affected in all cases except two (Williamson (11) cases 1 and 3).

(4) The sense of pressure, where noted, has been affected in all cases except one, viz., Dercum's case of absolute astereognosis from a tumour pressing on the medulla and cerebellum.

(5) In the great majority of cases, where the condition of the muscular sense has been tested in one or more of the usual ways, it has been found defective; in no case has the appreciation of weights been noted as normal; in two cases of absolute astereognosis the sense of position has been normal, in four cases lost, and in one disturbed; the sense of passive movement has been lost in three cases and normal in two; taken collectively, all the cases show defect of one or more forms of muscular sense, except three, in two of which only the sense of position has been noted, and in the third only the sense of passive movement.

(6) In no case, where noted, has ataxia of the affected arm been absent.

Any deductions, therefore, which can be drawn as to the relative importance of the various forms of sensation in stereognostic perception from the frequency and degree of their implication in cases of astereognosis, point to the sense of pressure, the tactile-localisation sense and the muscular sense (especially the faculty of appreciating weights), as the most important elements in the stereognostic sense. This result is evidently an incomplete one, as it does not prove the necessity for some degree, at least, of tactile sensation, which must of course exist. In this and, in fact, in all respects, fuller conclusions as to the peripheral components of the stereognostic sense could be drawn from cases in which there was defect of the elementary sensations, while the stereognostic sense was normal; there must be many such cases, although very few indeed seem to have been recorded.

It is of interest to note that in the various cases of astereognosis, in which note has been made of the fact, there has been a marked difference as regards ability to recognise the general physical qualities of objects handled. In the cases of Dercum (10) and Sailer (1), the patient could not give any in-

formation as to shape or other physical qualities of the objects; in the case of Olmsted (16), where the astereognosis was not quite absolute as the patient could not tell a knife by handling, the consistence of objects could be appreciated to some extent but not the shape; in one case of Williamson (11) (case 1), the patient had "occasionally some knowledge of the shape and hardness" of objects; in the case of Dubbers (5), although there was absolute inability to recognise objects by handling, the patient could almost always recognise their general qualities, *e.g.* size, length, shape, roughness or smoothness, hardness or softness. This point has been so seldom noted that it does not seem possible to form any definite idea from these cases of astereognosis as to whether this difference in individual cases bears any relation to the degree of impairment of the different elementary sensations or is of central origin; it is curious to remark, however, that, of the five cases mentioned above, the sensation of touch was normal in all except that of Dubbers, which yet retained far more than any of the others the power of telling the physical characters of objects.

We come now to the second problem connected with the stereognostic sense, viz., what is the nature of the cerebral process by which the various forms of sensation, which are factors in stereognostic perception, lead to the recognition of objects?

Dana (3) believes that "active touch" is dependent on "motor and tactile memories, associated with the special cortical motor centre for the particular part." He is a strong advocate of the view that the central convolutions of the cortex, although essentially motor, have also a sensory function, and that memories of sensations are also represented there. Thus he regards astereognosis as a tactile-motor amnesia, depending like motor paralysis on a lesion of the central convolutions. (v. Monakow (17) also thinks that especially the stereognostic sensations arise in the central convolutions.)

Many others, besides Dana, look on astereognosis as a memory-loss or amnesia. Thus Williamson (11) thinks it is analogous to word-blindness and word-deafness, and is "probably due to some cortical lesion"; he believes it probable that the special site of the lesion causing it is in the parietal region of the cortex just behind the arm centre. So Burr (2), who describes his case as *tactile amnesia*, thinks that isolated astereognosis is analogous to "mind-blindness" (*i.e.* the condition in which objects are seen quite well but are not recognised), the former being due to the loss of "tactile mental images" stored up from past experience, the latter to the loss of the stored-up results of previous visual stimuli; thus the condition is a

"memory-loss, or partial amnesia"; he thinks further that we may assume that "some definite area of the brain is concerned with tactile mental images, just as certain areas are concerned with mental hearing and mental vision, but where it is located we are at present entirely unable to say." Oppenheim (7) and Dubbers (5) also view touch-paralysis in the same light, viz., as a loss of the ideas or memories of objects derived from touch; the former states that this condition is common in the hemiplegias dating from birth or early childhood, because tactile memories have never been acquired with the paralysed hand.

Other writers, *e.g.* v. Monakow, Flechsig (7), etc., look on astereognosis as an associative disturbance, the result of defect in that correlation of the various elementary sensations, which is necessary for, and leads to correct stereognostic perception: these various sensations must be combined or correlated in the cerebral cortex before a mental picture of the object arises. Such defective correlation may, we may suppose, be due to a peripheral or a central lesion: in the one case, the elementary sensations are themselves defective and therefore their correlation is necessarily at fault, while, in the other case, the elementary sensations are intact and the sole defect is in their correlation. As an example of the first, we have Dercum's case of tumour pressing on one side of the medulla and cerebellum, causing impairment of the pain, localisation, and muscular senses: the author says "it is probable that the astereognosis was due to the partial sensory losses which interfered with the complete perception of the qualities of the object handled, and thus prevented a complete mental picture being formed in the cortex." On the other hand, cases of astereognosis with complete preservation of all the elementary sensations (*i.e.* isolated or pure astereognosis) are explained, on this theory, as due to lesions higher than the perceptive sensory centres—"somewhere in the higher (apperceptive) centres by which objects are recognised in consciousness" (Sailer); this, Sailer adds, "is not necessarily a definite group of cells."

Cases of isolated astereognosis have been but rarely recorded. Weir Mitchell (18) and Burr (2) have each recorded a case, combined with mind-blindness; Williamson's (11) second case and also—so far as the notes go—his fourth and fifth cases come under this category; in Sailer's (1) second case, it was noted that, four days after the onset of sudden left hemiplegia, there was great defect of all forms of sensation with astereognosis on the left side, while on the right side all forms of sensation were normal, but the stereognostic sense was extremely imperfect: as the right side showed a normal condition of the stereognostic sense as early as next day, its impairment must have been

simply an inhibitory effect of the sudden lesion which caused the left hemiplegia and hemianæsthesia — just as temporary total amaurosis may be caused by a sudden lesion in one cuneus.

The recognition of an object by handling must, I believe, imply a correlation of the various sensory impressions evoked by the act of handling; we may deduce, from analogous examples, that the anatomical basis for this is the presence of association tracts between the various perceptive sensory centres (tactile, muscular, etc.). Just as in the case of other forms of sensation, we must assume that there is the power of storing up and reviving past "stereognostic" impressions—that there is a stereognostic concept of objects, just as there is an auditory or a visual concept—and that any new stereognostic impression is fully apprehended only by comparison with former stereognostic impressions, with the help of "memory." But the further question arises: Does the recognition of the nature of an object by handling require anything more than a correct stereognostic concept? Some help may be got towards answering this question by considering a few analogous problems and the solutions suggested for them.

Isolated astereognosis has been, we have seen, frequently compared to mind-blindness or word-blindness: in the one case objects, in the other case words, are seen but are not comprehended. The most probable explanation of the condition known as mind-blindness, or better, object-blindness, seems to be that given by Lissauer, Bastian (19), and others: the recognition of an object by vision requires not only intact visual impressions but also the associated action of other sensory centres (notably those connected with ocular movements—Lissauer), and object-blindness is due to interruption of the tracts connecting the common visual centres with these other sensory centres: "a failure in the irradiation of associational impressions which are wont to pass from the visual to other sensory centres on the presentation of an object to the visual sense would, of course, absolutely check the neural processes essential to perception, and thus lead to the non-recognition of the object and its uses" (Bastian).

Two conditions, connected with the speech-mechanism, deserve mention in this connection. Freund has described as *optic aphasia* a condition in which objects are seen and recognised, but they cannot be named at sight. It is generally believed that this is due to interruption of the optic-acoustic connections, *i.e.* the tracts connecting the visual centres with the auditory speech centre, the lesion—often a cerebral abscess due to ear disease—being situated about the junction of the left

occipital and temporal lobes. A much rarer condition is what is known as *tactile aphasia, i.e.* inability to name objects by handling them, although their nature is quite recognised. Vorster (20) records such a case, and finds only eight others recorded in literature: in every case tactile aphasia was combined with optic aphasia—a suggestive fact with reference to the pathology of tactile aphasia, and one in accordance with the view held by writers on the subject (Moeli (21), Oppenheim (22), Vorster (20), etc.), that, in naming many objects by handling, especially those in which the visual component is a very strong factor in the concept of the object, the visual memory of the object must be called up before it can be named.

The same associated activity of the visual centres is held by Kattwinkel (23) to be necessary for recognising and naming letters written with a blunt object on the skin; not only are correct tactile and pressure perceptions necessary, but these impressions must be transferred from the tactile centre to the centre for the visual memories of letters—"inward sight" is necessary. This conclusion is, he finds, confirmed by examination of cases of aphasia. This faculty was quite intact in cases of pure motor aphasia, while it was abolished in cases with sensory aphasic defects pointing to lesions in the visual centres.

Thus, the common principle assumed to underlie many cases of such defects as object-blindness, tactile aphasia, and inability to name letters written on the skin, is this, viz., the sense, to which the object or letter is presented, is incapable of giving full recognition without the associated action of other sensory centres.

This principle must also be recognised in considering the stereognostic sense. In the conception of external objects generally, the most potent factor is certainly not the stereognostic but the visual. There is, no doubt, a difference according to (1) the nature of the object, especially with regard to the sense to which it is usually presented, and (2) individual peculiarities in different persons; but, with regard to objects generally, it may be said that the vast majority of normal people are preponderatingly "visual," just as they are "auditory" with regard to words. It is true that the relative importance of the different factors in the concept of an object may be altered by special training and in special circumstances. Thus, a congenitally blind person may recognise objects by handling, although there are no visual impressions at all; so, in normal people, where an object has never been presented to any other sense than the stereognostic, this sense may, with practice, be able unaided to recognise it, and it is possible that this is true, in actual experience, of certain habitually handled objects. But in

dealing with cases of astereognosis, occurring as the result of a definite lesion of the nervous system in persons hitherto normal, we have to consider not what may be the result of special training, but the way in which the recognition of objects is habitually brought about. It may be true, as Goldscheider (24) holds, that objects may be *named* by means of simple reflex action between any one perceptive centre and the speech centre, without any concept of the object being necessary; but personal experiment seems to prove that, in the case of the vast majority of objects, the visual memory of the object is distinctly called up before its nature, and uses can be recognised by handling, *i.e.* the impressions got from handling an object are transferred to the visual centres before the object is recognised. If this be so, it is evident that isolated astereognosis may be caused by a lesion interrupting the tracts which connect the various sensory centres concerned in handling with the visual centres, or implicating the visual centres themselves. Some confirmation of this view is got from examination of the few recorded cases of isolated astereognosis. Thus, in the cases of Weir Mitchell (18) and Burr (2), there was also a defect of sight apparently of the nature of object-blindness, and in Mitchell's case also hemianopia; these symptoms would point to lesions in the neighbourhood of the visual centres. Again, in Williamson's case 2 (11), the site of tenderness on the skull corresponded fairly well to the region of the angular gyrus, and there was evidently some indication of hemianopia.

The subject seems to me of sufficient interest to point to the importance of including the stereognostic sense in the routine examination of the sensory condition in cases of nervous disease. The absence as well as the presence of any disturbance of this sense should be noted, as the absence of astereognosis in cases with defect of certain elementary sensations would probably be more valuable than its presence in determining the importance of the various peripheral elements which contribute to stereognostic perception. In order to determine the nature of the central process, it would be important to note, in cases of isolated astereognosis, the presence or absence of symptoms pointing to disturbance of other sensory centres, especially the visual. The process denoted by the stereognostic sense, whatever its details may be, is evidently an associative one, and hence we may expect that such a process, being complex and specialised, should be much more readily disturbed than perception of the simple sensations, as the effect of a sudden cerebral lesion or a general mental defect; hence astereognosis will more readily emerge and more slowly disappear as an indirect symptom of cerebral mischief than simple loss of touch or pain. We must, therefore, be very cautious in drawing

deductions as to the stereognostic sense from cases of sudden cerebral lesion until it is reasonably certain that all indirect effects have passed away.

The conclusions, to which the considerations detailed above lead, may be summarised thus :—

(1) The stereognostic sense is not a simple sense, but depends on the complex of sensations derived from the act of handling an object. These various sensations are correlated in the cerebral cortex. This correlation appears, at least in the majority of objects, to require the associated activity of other sensory centres, especially the visual, before it can lead to the recognition of the nature of the object and its uses.

(2) Astereognosis may be due to a peripheral or a central lesion.

(3) Peripheral astereognosis is due to a lesion of the sensory nerves or their perceptive centres: in this case there is defect of one or more of the elementary senses. So far as cases of astereognosis show, the pressure-, localisation- and muscular-senses are the most important elements in the stereognostic sense.

(4) Central astereognosis may be due to one of two causes:

(α) Defective correlation of the various sensory impressions which are factors in stereognostic perception.

(β) Interruption of the tracts connecting the site of this correlation with other sensory centres (especially visual) whose simultaneous activity is habitual in recognising objects by handling.

In both (α) and (β) the elementary sensations may be intact.

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DISEASES OF THE KIDNEYS AND DIABETES

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DURING the last few months considerable prominence has been given to a discussion on the question of diet in chronic interstitial nephritis. Von Noorden ("Verhandlungen des Congress für innere Medicine," 1899, p. 386), in a paper read before the Congress for Internal Medicine, points out that since the question had been discussed under the guidance of Senator and Ziemssen in 1890, it had not come prominently under the notice of German physicians. The scheme usually laid down consisted in a diet moderate in foods rich in albumen, with an exception in favour of vegetable albumen and the albumen of milk, abundance of fresh and cooked vegetables, and carbohydrates of different kinds. The quantity of fat and carbohydrate varied with the state of nutrition of the patient. Milk was allowed in large quantities; alcohol, tea, and coffee were more or less vigorously restricted, while alkaline mineral waters were usually prescribed.

In the diet only *white* meats were permitted, brown or red flesh, even of game and birds, being entirely forbidden by many physicians. Von Noorden considers that this prohibition has disastrous results in many cases, the patient developing a disgust for all meat, which results in a deprivation of albuminous foods, diminished nutrition, and weakness.

There appears to be no exact chemical data for the prohibition of red meat; it seems to be founded on the supposed occurrence in brown meat of a large proportion of substances which have an irritating action upon the kidneys, especially the nitrogenous extractives. This, however, is not borne out by the analysis of the food stuffs. The amount of creatin in the flesh of fowl or rabbit reaches 3·4 per mille, while in ox flesh 3 per

mille is never reached. The real difference between white and brown flesh consists in the proportion of colouring matter present, the chemical composition of which colouring matter we do not know, and for the evil effects of which we have no evidence. The author quotes an observation in support of his contention.

Offer and Rosenguist (*Berliner klin. Wochenschr.*, Nos. 43 and 44, 1899), in a careful analytical paper, give tables showing the analysis of the different forms of flesh and their proportional values in nitrogen, extractive nitrogen, and extractives. The authors find that they cannot support the theoretical difference between red and white flesh and their use in the diet of patients suffering from chronic interstitial nephritis and gout. The exclusion of red meat from such diet schemes they find based upon an unsupported prejudice which renders the satisfactory dieting of the patient more difficult. They discuss the composition of the different meats fully, and ground their conclusions on the analysis.

Senator (*Berliner klin. Wochenschr.*, No. 45, 1899) enters a protest against these views, and says a word in favour of the time-honoured custom of excluding red meat from the diet of interstitial nephritis and gout. The custom he considers time-honoured not without reason, and supported by a mass of clinical evidence and the weight of practical experience.

Von Noorden, in the article above referred to, strongly advocates the limiting of the ingestion of fluids in cases of chronic interstitial nephritis, when heart failure and œdema have supervened. He quotes cases where a limited ingestion of fluid has resulted in marked diuresis and the establishment of compensation, while on a return to milk diet a failure of compensation resulted.

Under the title of "Hæmorrhage from Anatomically Unchanged Kidneys," Grosgklik (*Sammlung klinische Vorträge*, N.F., No. 203) records a case of the so-called renal hæmorrhage of angio-neurotic origin, and discusses the literature. The author concludes that of the seventeen recorded cases only fifteen can be accepted. In most of the cases the cause was unknown, or else the hæmorrhage occurred in the course of hæmophilia. Klemperer (*Deutsch. med. Wochenschr.*, 1897, Nos. 9 and 10) classifies these cases as they occur as the result of (1) hæmophilia, (2) physical strain following trauma, (3) a general angio-neurosis of vasomotor origin. Of the vasomotor type, Klemperer records two cases, the diagnosis being arrived at by exclusion. Poljakoff (*Deutsch. med. Wochenschr.*, No. 44, 1899), from a careful study of the literature, concludes that in a very small number of cases has the diagnosis been established in a

satisfactory manner. He records a case which came under his own notice, where the bleeding seemed undoubtedly to be due to vasomotor disturbance. The patient, a young woman, suffered from mitral insufficiency, which, however, was completely compensated. She suffered from migraine and on mental disturbance from angio-neurotic œdema. The patient was suddenly seized with hæmaturia and urinary distress. The urine contained blood and casts. The hæmaturia lasted twenty-four hours and then the urine became normal again. The left kidney was tender and palpable. Various organic disorders were discussed only to be dismissed. The angio-neurotic phenomena elsewhere being taken into account along with the other symptoms as migraine, the author concluded that the attack was angio-neurotic in origin. For a year and a half the patient remained well, then suddenly, during a period of mental disturbance, hæmaturia recurred and persisted for some days. It recurred at intervals under mental excitement, always to disappear under rest and appropriate diet. It is noteworthy that the hæmorrhage seemed to be from the left kidney which was palpable and tender, while the neuralgic attacks from which the patient suffered always affected the left side. The hæmorrhage, the author ascribed to a paralytic affection of the vaso-motor nerves resulting in diminished tone in the vessels and an overfilling of the capillaries.

There are no recent papers of importance in the therapeutics of renal disease. Bronowski (*Wiener med. Presse*, 1899, No. 5), from observations on animals, finds that strontium lactate causes a general lowering of the blood pressure. If more than a grain per kilogramme of body weight be given, renal irritation is induced. The drug acts as a diuretic, not by direct influence on renal epithelium, but by dilatation of the renal vessels. Clinically, Aronowski finds that when the drug is exhibited, diuresis occurs in the majority of cases, the diuresis being accompanied by a diminution in the amount of albumen in the urine. Thomson (*Boston Medical and Surgical Journal*, Nov. 1899), discussing the drugs at present in use in irritability of the urinary tract, concludes in favour of urotropin, which has given very favourable results in his hands. Given in seven and a half grain doses morning and evening or thrice daily, it has a marked influence as a urinary antiseptic and in rendering alkaline urine acid. No toxic effects and no marked diuretic influence were observed.

The condition known as bacteriuria has attracted a good deal of attention in the last few months. Qredöhl (*München. med. Wochenschr.*, Nov. 1899, p. 1495) contributes a paper on a typical case with a resumé of the literature of the subject.

Warburg (*Ibid.*, No. 29) finds that in 65 cases so far recorded, 22 cases showed the presence of sarcinæ, 17 cases B. Coli, 3 cases Staphylococci, 2 cases B. Coli and Staphylococci. In Warburg's case the symptoms were rigors, fevers, malaise, etc., in a previously healthy individual. The fever lasted for some days, to subside however before the urine cleared under the use of urotropin. Nicolles (*Canadian Journal of Medicine and Surgery*, Dec. 1899), discussing the etiology of chronic Bright's disease, concludes from a considerable study of microscopic sections that all forms of nephritis are due in the majority of cases to infective agents, the acute to the usual specific germs of the primary disease, and the chronic, as a general rule, to the B. Coli, though other germs may sometimes be concerned. The point of invasion of the B. Coli is the gastro-intestinal tract; for other germs it may be various.

An interesting paper is contributed by Gwyn (*Johns Hopkins Hospital Bulletin*, June 1899), on the presence of typhoid bacilli in the urines of typhoid fever patients. Gwyn finds that in twenty to thirty per cent. of all cases of typhoid fever, typhoid bacilli are found in the urine. When present they are usually in pure culture, often so numerous as to be demonstrable by a cover slip examination. The organisms usually appear in the second or third week and may persist for months or years, probably multiplying in the bladder, the urine being apparently a suitable medium for their growth. Though often showing evidences of cystitis and marked renal involvements, the urine containing bacilli has usually only the characteristics of an ordinary febrile urine. The presence of the bacilli has no prognostic significance. Irrigation of the bladder with bichloride of mercury and the internal administration of urotropin seem to be safe methods of removing the bacilli. The author draws obvious conclusions as to the necessity for disinfection of the urine in typhoid fever.

Leo (*Deutsch. Med. Wochenschr.*, No. 43, Oct. 26th, 1899), discussing the nature of diabetes mellitus, points out that by means of experimental work, v. Meering and Minkowski have arrived at the conclusion that in health the pancreas has a special function in relation to the metabolism of sugar, and that it is in the deprivation of this function that the cause of diabetes is to be found. It is, however, well recognised that there are many cases of diabetes in which the pancreas is found *post-mortem* intact. Leo offers an explanation of this apparent contradiction. He postulates that in those cases where there is no pancreatic degeneration there is a toxic agent which, incubating in the blood, checks the metabolism of sugar in the tissues. He considers that if this toxic agent were in solution in the

circulation, on its injection into an animal glycosuria should be produced. His efforts to obtain this result have so far proved unsuccessful. He carried out observations by administering diabetic urine to animals, but with contradictory results. Ascitic fluid, from a case of diabetes, when injected gave negative results. The injection of urine subcutaneously and intravenously gave variable results. With urine from cases of mild diabetes negative results were obtained, with cases of more pronounced diabetes, a slight glycosuria followed. Check experiments with non-glycosuric urine were negative in result.

SURGERY

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Operations on the Sympathetic Nervous System.—Although the connections and functions of the sympathetic division of the nervous system have long been known to physiologists, and the rôle played by its different ganglia and plexuses in the production of certain diseases or symptoms has at least been suspected by pathologists and physicians, it is only within very recent times that the surgeon has brought this mysterious and inaccessible structure within his sphere of influence.

Since Gerard-Marchant (1), Jaboulay (2), and Jonnesco (3), about the same time, carried out operations on the great sympathetic cord in the neck for the relief of the more prominent symptoms of exophthalmic goitre, similar procedures have been suggested and practised for the treatment of such widely different conditions as epilepsy, trifacial neuralgia, glaucoma, abdominal and pelvic neuralgias, sciatica, and hysterical arthropathies.

The time which has elapsed since these operations have been performed is as yet too short to justify conclusions as to their real value, but a sufficient number of cases are now on record to enable us to appreciate the present-day position of this branch of surgery, and to furnish us with a standpoint from which to watch its further progress.

Exophthalmic Goitre.—While in a considerable number of cases removal of the whole or even of one-half of the thyroid gland has yielded more or less satisfactory results in cases of Grave's disease, the effects are so inconstant, and in many cases the benefit so transient, that many surgeons hesitate to recommend so serious an operation of which they can promise so

little. The close correspondence between the more prominent clinical features of this disease, and the well-known physiological effects of stimulation of the cervical sympathetic in the neck, suggested to Jaboulay that division of these cords might at least diminish the exophthalmos and palpitation. In nine cases so treated by him the results were highly satisfactory.

Gerard-Marchant, Jonnesco, Schwartz (4), and others, have met with equal success after the more radical procedure of resecting the whole of the cervical cord with its ganglia on both sides of the neck. That this operation is not unattended with risk is shown by Faure's experience. During the operation in one case there was dangerous syncope; and in another the patient died suddenly from heart failure, while the operation was being completed. It must be allowed, however, that such accidents are very liable to occur in this class of patients during any operation involving general anæsthesia. Faure suggests that ether should be used as the anæsthetic, and that only one side should be operated upon at a time. It is not clear, however, that this will diminish the sum-total of risk, because a second administration will be necessary to complete the operation, and this, together with the mental anxiety on the part of the patient, in anticipation of a second operation, may more than neutralise the benefit of the shorter and less severe sittings. On the other hand it is possible that removal of one cord may suffice to cure the condition, as it is well known that the symptoms of Grave's disease may disappear after any operation, after a severe mental shock, or even spontaneously.

An excellent description of the technique of the operation is given by Juvara (5), who was one of the first to publish a successful case. The head is turned to the side and the neck stretched over a rounded cushion. The incision extends from the junction of the inner third of the clavicle with the outer two thirds, along the posterior border of the sterno-mastoid, in the direction of the apex of the mastoid process, as far as the level of the angle of the lower jaw. In the lower part of the wound the external jugular vein is seen, and at its upper angle the transverse cervical and auricular nerves are exposed, and should be saved to avoid a disagreeable anæsthesia of the parts supplied by them. The sterno-mastoid is raised and pulled forward, and the scaleno-vertebral plane of the neck thus opened up. The scalene tubercle of the sixth cervical vertebra is then recognised, and on it lies the reddish grey cord of the sympathetic. This is seized with forceps and traced upwards and downwards. The superior ganglion is reached by retracting the upper angle of the wound and dissecting with a blunt director behind the sterno-mastoid and the sheath of the large vessels. When isolated it

is caught with forceps and twisted out. The inferior ganglion lies in the scaleno-vertebral triangle behind the vertebral artery, and when exposed it is also seized with forceps and with the middle ganglion, and the cord connecting them is removed. There is sometimes slight bleeding after isolating the superior ganglion, due to tearing of a small branch of the ascending pharyngeal which crosses it. It is readily arrested by pressure. Any hæmorrhage occurring during removal of the inferior ganglion must be treated with great care, and no forceps must be used until the actual bleeding point is clearly seen, otherwise the vagus or phrenic nerves, or on the left side the thoracic duct, may be damaged. Partial resections or simple division of the cord may be carried out in the same way.

Epilepsy.—While the results of surgical intervention by trephining in cases of Jacksonian or focal epilepsy have been sufficiently satisfactory to justify surgeons in continuing the practice, the same can scarcely be said of true or generalised epilepsy. Nor have the results of ligature of the vertebral arteries been any better. So far as published results guide us at present the outlook from excision of the cervical sympathetic is brighter. Jonnesco (6) reports a series of forty-three cases treated in this way with satisfactory results. Chipault (7) adds other eighteen, and many others have recorded smaller numbers. It is true that in many of the cases the condition has only been alleviated, and that in many more the operators have been in such haste to publish their cases that they prove nothing beyond the fact that the operation is possible and not necessarily fatal; at the same time there is a considerable residuum of sound surgical success.

The operation is based on the acknowledged influence exercised by the sympathetic on the cerebral circulation, and the theory that the epileptic attacks are due to toxic substances circulating in the blood. Chipault believes that the resection of the cervical sympathetic undoubtedly acts by provoking a permanent "lavage" of the brain, so ridding it of these noxious elements. E. Vidal (8) has recently carried out some investigations on the physiological basis of the operation, to meet objections which have been raised to it on purely physiological grounds. He injected various irritants capable of producing epileptiform attacks in guinea-pigs, and found (1) that an intoxication insufficient to provoke an attack, becomes sufficient, if at the same time the cervical sympathetic is electrically stimulated. (2) That an intoxication insufficient to provoke an attack in a normal animal, becomes sufficient after ligature of one or more of the arteries supplying the brain. (3) That

anæmia of the brain artificially induced diminishes the dose necessary to produce convulsions, while congestion increases the dose. (4) That if a vaso-dilating substance be injected with the irritant a larger dose is necessary. (5) That complete excision of the cervical sympathetic increases the resistance of a healthy animal to certain irritants. From these and other experiments he concludes that the operation is justified on physiological grounds.

Trifacial Neuralgia. — The trifacial nerve is very rich in sympathetic fibres derived from the superior cervical ganglion, and from the carotid plexus, and it has been proved that ablation of the superior cervical ganglion brings about certain secondary degenerations in the cerebro-spinal nerve centres. In view of the success which followed operations on the pelvic sympathetic plexuses in cases of neuralgia, Termier (9) and Jaboulay have treated trifacial neuralgia on the same lines. In a case recorded by Termier, a man of 60, who had for long suffered from intense paroxysmal neuralgia which had resisted all treatment, had his superior ganglion excised, with the result that the pain was at once relieved and four months later was still absent.

Glaucoma.—In glaucoma due to irritation of the vaso-dilator nerves derived from the cervical sympathetic, little benefit seems to follow on iredectomy. Ruggi (10) has reported ten cases, and Chipault three, in which the superior ganglia, sometimes only on one side, sometimes on both, have been excised for this condition. In a majority of the cases, including one of Chipault's which was of the hæmorrhagic variety, there has been a diminution of pain and of tension in the globe, with improvement in vision. The operations are all too recent to justify conclusions as to the ultimate results.

Abdominal and Pelvic Neuralgias.—That many severe neuralgias of the pelvic and abdominal viscera are of sympathetic origin has long been believed, in view of the character of the pain, the vaso-motor phenomena which so often accompany the paroxysms, and from anatomical considerations. Jaboulay (9) has operated for such conditions as vaginismus, hysteralgia, neuralgia of the uterine adnexa and other pelvic pains, by removing the sacral plexus of the sympathetic. The operation consists in making an incision 4 inches long, along the border of the sacrum, dividing everything down to the edge of the great sacro-sciatic ligament. This is then partially divided, and the rectum is separated by blunt dissection from the front of the sacrum. The cavity is packed tightly with gauze till all oozing ceases, and then are seen the anterior fibres of the sacral sympathetic which go to form the hypo-gastric plexus. These

together with the fibres of communication with the spinal sacral nerves are destroyed.

Some of the results are so striking that they may be cited. A nervous but otherwise healthy woman of thirty-nine suffered from severe vaginismus, with painful menstruation. For five years the pain had been continuous, with monthly exacerbations. On the 18th of November 1898 the operation was performed. Next day the vaginismus had gone, and the day after menstruation came on painlessly. In January of this year she was regular, healthy, and free of pain. Another patient, aged twenty-six, had a similar experience. It is worthy of note that in nearly all the females operated upon for this or other forms of pain, menstruation began within a day or two of the operation, doubtless as a result of the vaso-dilatation following paralysis of the sympathetic.

Ruggi (10) has obtained similar results in such cases, but he operates by laparotomy, reaching the renal, aortic, spermatic, or pampiniform plexuses by the abdominal route.

For certain forms of intense visceral neuralgia, with throbbing of the aorta and distension of the abdomen, Jaboulay has performed a median laparotomy, exposing the pylorus and pulling it forward, so as to reach the front of the aorta in the vicinity of the cœliac axis and superior mesenteric arteries. The finger or a blunt director passed along the front of the aorta is sufficient, he says, to affect (*impressioner*) the branches of the solar plexus. He claims to have had satisfactory results.

Sciatica and certain *hysterical arthropathies* of the lower extremities, associated with vaso-motor disturbances, have been treated by exposing the sympathetic in the hollow of the sacrum with success. In some of the cases recorded the sciatica, which had lasted for years and had resisted all forms of treatment, including nerve-stretching, disappeared at once and had not returned some months after the operation.

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Coxa Vara.—There is much in a name, even if it be the name of a surgical disease. In the beginning of 1896 Professor Ogston of Aberdeen published a paper, the material for which he had been collecting for years, on a condition which he called "Rachitic Deformity of the Head and Neck of the Thigh Bone." But in

spite of the extreme interest and importance of this paper, it attracted little attention until the condition described in it was identified by the more euphonious, if less descriptive, term of "coxa vara," a name it owes to Hofmeister of Tubingen. These two workers had independently recognised the peculiar deformity of the upper end of the femur which characterises this condition, although Hofmeister was the first to publish his results, but even he had been anticipated by Ernst Müller in 1889.

Within the last eighteen months many cases have been placed on record confirming the accuracy of the descriptions given by these observers, and adding to our knowledge of its pathology and clinical importance.

In coxa vara the head of the femur becomes depressed so that it lies as low, or even lower than the top of the great trochanter, and at the same time the neck of the bone becomes abnormally curved in a forward direction. The trochanter is prominent and passes above Nelaton's line. The diminution of the normal open angle of the femoral neck leads to shortening, and the forward curving of the neck causes eversion of the limb, so that the patient limps on walking, and is unable to kneel or to sit with comfort, the disability being out of all proportion to the apparent deformity. Abduction of the limb is markedly diminished, and full flexion is only possible if at the same time the limb be abducted and rotated out.

The clinical importance of this condition lies in the fact that serious errors in diagnosis may arise from failure to recognise its true nature. Thus it is liable to be confused with *congenital dislocation of the hip*, as is well illustrated by Muirhead Little's experience. This surgeon, with great frankness, reports three cases diagnosed as coxa vara, which, when subjected to the crucial test of the Rœntgen rays, turned out to be cases of congenital dislocation. This mistake is easily made, as both conditions may be met with in young children, may be uni-lateral or bi-lateral, and present clinical features closely resembling one another. Some writers lay stress on the possibility of lengthening the limb by traction in cases of congenital dislocation as a point in differential diagnosis, but others show that this symptom is by no means constant. In coxa vara, flexion and abduction are always restricted.

A rachitic curvature of the upper end of the shaft of the femur may simulate either of these conditions.

Still more important, although no less difficult, is it to differentiate between coxa vara and early *coxitis*. In the latter condition there is flexion and abduction of the limb, and pain is often referred to the back or inside of the knee. In coxa vara

the limb is straight and abduction is diminished, and as has been pointed out by Stoker, when pain in the knee is present it is referred to the front of the joint, being transmitted by the anterior crural nerve. The absence of tenderness, local swelling, muscular atrophy and reflex fixation of the limb further differentiate it from disease of the hip joint. Of course it is not impossible, or even uncommon, for tubercular disease to be superadded to coxa vara, the depressed vitality of the patient, locally and generally, predisposing him to such a combination.

In the absence of a history of injury, fractures or traumatic dislocation may be readily excluded.

The somewhat rare condition of upward enlargement of the acetabulum must not be left out of mind.

The final appeal in all cases of diagnostic difficulty is to the Rœntgen rays.

When the disease occurs in young children or adolescents it is usually to be attributed to rickets; when in those beyond puberty, there is no evidence of rickets, and the condition is due to osteo-malacia, inflammatory or traumatic lesions of the bone. It occurs oftener in males than in females. In 25 per cent. of Frazier's cases other deformities in the form of genu vulgum, or flat foot, were present. Prolonged standing and the bearing of heavy weights tend to aggravate the condition.

The *treatment* is in the first instance constitutional, special attention being paid to the rachitic diathesis, or to such other general condition as may underlie the process.

In the early stages of the disease the patient must be laid up in bed to relieve the neck of the femur of the body weight, which has produced and is increasing the deformity. At the same time some mechanical apparatus must be applied which will combine inversion with extension of the limb.

Should this line of treatment fail, in the case of young children the shaft of the femur may be divided below the lesser trochanter, through an incision carried along the outer aspect of the bone. After the position of the limb has been corrected the bone may be clasped by an aluminium ferrule (Watson-Cheyne), and the wound closed. The limb is then fixed in a posterior splint, and laid in the position of internal rotation. The patient should not be allowed to use the limb for walking for two or three months.

In young adults the best results seem to follow the excision of a wedge-shaped portion of the neck of the femur, the base of the wedge being at the upper margin of the neck (Kraske).

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OBSTETRICS AND GYNECOLOGY

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Hæmostatics in Gynecology.—The search for the perfect hæmostatic is still continued. In this respect it resembles the search for the perfect anæsthetic. Perhaps in the end success will reward the diligent searchers in both cases; in the meantime most of us still use—*faute de mieux*—ergot and chloroform. Stypticin is one of the hæmostatics of which much was expected, and from which not very much has been obtained. It is (chemically) cotarnine hydrochloride, and cotarnine is methyloxyhydrastinin, and it is one of the oxidation products of the opium alkaloid narcotin; also it was introduced into gynecological practice by Freund. It is closely allied to hydrastin, but was supposed to possess the advantage over that uterine hæmostatic in that it had a distinctly sedative action. So it was expected that it would be of great value in dysmenorrhœic menorrhagias. It was also used largely by Gottschalk and others in sub-involution (without retained membranes), in the bleeding from fibroids, and in climacteric uterine hæmorrhage; and it was thought that it would largely take the place of curettage for endometritis in the case of patients who objected to operation. It was regarded as contra-indicated in threatened abortion and in the hæmorrhages of pregnancy on account of its supposed action on the uterine muscle. It was given in doses of 0·016 to 0·032 grm. ($\frac{1}{4}$ to $\frac{1}{2}$ grain). Soon it began to be found that it could be given in hæmorrhage in pregnancy without exciting uterine contractions, and that in fungous endometritis it was best to trust to the curette rather than to stypticin given either hypodermically or by the mouth. Further it began to be stated that it had no vaso-constrictor action, and that it did not hasten the coagulation of the blood (Marfori). Recently Abegg (*Centrlbl. f. Gynäk.*, Bd. xxiii, s. 1333, Nov. 4th, 1899) has reported further on stypticin; he found it useless in the hæmorrhages due to

large uterine fibroids or polypi, but of some value in bleeding in the puerperium, and of great value in climacteric metrorrhagia; it was of no use in threatened abortion; and finally, it found its greatest sphere of usefulness in the cases in which in addition to the hæmostatic effect it was desirable to produce quiet sleep, and it had the advantage that it did not excite nausea and vomiting as did ergot. Abegg gave it in doses of 0·05 grm. twice to five times a day. EDMUND FALK (*Monatsschr. f. Geburtsh. u. Gynæk.*, Bd. x., s. 484, 1899) has added still further to our knowledge of the pharmacology of stypticin. He found from experiments on pregnant rabbits and rats that the drug had no effect on the uterus in the way of exciting contractions. The hæmostatic influence of stypticin, therefore, could not be a consequence of uterine contractions. Any increase in the blood pressure which may follow its use must be regarded as secondary, and due to its action on the respiration. It was important to determine the direct effect of stypticin upon the blood-vessels, and so Falk examined under the microscope the tongue of a frog under whose skin a dose (0·01 grm.) of cotarninum hydrochloricum had been injected; no contraction of the vessels themselves took place, they remained of their original calibre, but there was a slowing of the blood stream. Under the same conditions hydastinin produced no slowing of the stream, but after eight minutes there was a very evident narrowing of the larger and smaller vessels, so much so that some of the smaller ones became invisible. This experiment shows that whilst hydrastin stops hæmorrhage by its effect on the vessels, stypticin can only do so by slowing the blood stream. If it be remembered that the latter is a derivative of narcotin, an opium alkaloid, it is not difficult to understand how it should check bleeding by its influence on the nervous system. In this respect it resembles not only opium but also belladonna and hyoscyamus, which the older physicians employed for bleeding in nervous, excitable individuals. Falk concludes then that cotarnin acts as a hæmostatic, not because it is chemically allied to hydrastin, but because it is a narcotic. Stypticin, then, is not a styptic ("keineswegs ist aber stypticin ein stypticum"), and in order to prevent disappointment in future it will be well to call it cotarnin. It will be found useful, therefore, in a limited number of cases of bleeding: in hæmorrhages in which no pathologico-anatomical cause is to be discovered, as in anæmic girls, in whom menstruation is profuse and accompanied with pain; in climacteric hæmorrhage, in which no endometritis or morbid state of the genital organs exists. In the case of bleeding from fibroids its action was irregular; and in some instances of hæmorrhage following disease of the

uterine annexa it seemed to do well, while in others, apparently identical, it failed entirely.

Another hæmostatic, about which a great deal has been written lately, especially in France, is gelatin. J. B. NICHOLS (*Medical News*, of New York, lxxv., 705, December 2, 1899) refers to its use, either by subcutaneous injection or local application, in a large number of forms of hæmorrhage, including the internal ones, such as aneurisms, hæmoptysis, hæmaturia, etc.; but it can hardly be looked upon as safe when injected subcutaneously, and it is with its action in gynecology that we are here specially concerned. A solution is employed consisting of from 5 to 10% of gelatin with ·6 to 1% of calcium or sodium chloride in 100 parts of water; this may be sterilized by heating to 100° C. for fifteen minutes daily for two or three days. Nichols has collected together thirty-two cases in which it was used in uterine hæmorrhages. The cause of the bleeding was chronic metritis in fourteen cases. It was metritis with a mucous polypus in one, abortion in seven, a fibroid in six, cancer of the cervix in one, and laceration of the vagina during labour in one. In all these the use of the gelatin solution was described as successful. In another case the cause of the bleeding was the menopause, and in yet another the cause was unknown; in these two it was unsuccessful. The case in which the author obtained the most marked good effect was not gynecological: it was that of persistent bleeding or rather oozing after an accidental wound in an individual, presumably of hæmophilic tendencies; all ordinary means, surgical as well as medical, had been tried, and the use of the gelatin was followed by a cessation of the hæmorrhage and by recovery. In uterine hæmorrhage the solution might either be injected into the uterus or applied as a gelatinised tampon. It is noteworthy that Bertino (*Annali di ostetricia*, xxi. 833, 1899), although highly esteeming gelatin in obstetric and gynecological hæmorrhages, admits its defects, *e.g.* difficulty of preparation and tendency to become infected by microbes.

Purpura in Pregnancy.—Among the rarest of the complications of pregnancy must be reckoned purpura hæmorrhagica. J. C. Diehl's case, therefore, would be well worthy of notice, even if it were less fully reported than it is (*Zeitschr. f. Geburtsh. u. Gynäk.*, xli. 218, 1899). It was that of a woman, thirty-six years of age, who had had six normal confinements and no abortions. She was again pregnant and had reached the fifth month when pains and stiffness of the lower limbs set in, and she was compelled to keep her bed; she gave birth to a male fœtus measuring 25 cm., and the confinement was not accom-

panied by marked bleeding. The succeeding morning, however, she had a severe attack of vomiting, during which the placenta was expelled along with a large quantity of blood. Some hours later she felt pain at the umbilicus, and when the binder was removed a discoloured area about the size of a five mark piece was discovered; at the sides of the body were similar patches. The patient was then brought into Professor Veit's Hospital. The discolorations were distributed in a somewhat symmetrical fashion: there was one round the navel, one in the middle of the back, one at each side over the lower ribs, one at each side over the clavicles, and smaller ones scattered over other parts. They were of a blackish blue colour and were clearly marked off from the surrounding skin, above the level of which they projected slightly. They felt hard and were somewhat painful on pressure. No hæmorrhages were noticed on the visible mucous surfaces of the head, but the labia majora and minora were much swollen, and of a black blue colour. The pulse was small, soft, and quick. The gums showed nothing abnormal, the heart was not enlarged, and there were no murmurs. The spleen was of normal size, and the uterus reached to the umbilicus. There was a slight degree of ascites; the urine contained blood, albumen, indican, a little urobilin, no bile pigments, and, microscopically, showed hyaline casts, white and red blood corpuscles and epithelium. Some blood examined was paler than usual, but otherwise normal. The patient suffered from pains, requiring morphia; the urine had to be drawn off, and some of the fœtal membranes were removed from the vagina, and douches given. She rapidly became worse, the hæmorrhagic patches increased in number, there was fæcal incontinence, and death occurred on the third day of the puerperium. The fœtus, like the mother, showed cutaneous hæmorrhages, with a somewhat symmetrical distribution, but affecting only the head and back; they were punctiform. The necropsy of the mother revealed numerous internal hæmorrhages in the dura mater, under the periosteum of the anterior wall of the spinal canal, in the peri-aortic tissues, in the left crus of the diaphragm, in the tissue round the urethra, vagina and cervix, and in the bladder wall, etc. The fœtus showed, at the time of autopsy, irregularly distributed hæmorrhages in the dura mater, in the spinal canal, in the visceral layer of the pericardium, in the intestinal peritoneum, and on the palate; there were no signs of syphilis. The microscopical appearances of the maternal and fœtal organs are given in detail, but it must suffice to note that in none of the organs (maternal or fœtal) were micro-organisms discovered although they were carefully looked for. Diehl discusses at great length the various causes of the changes in the vessel walls which produce purpura,

but is compelled to leave the cause in the case just recorded uncertain. He concludes that almost all cases of purpura are secondary in their nature; only few cases can be regarded as idiopathic, and possibly even they are due to an organic or functional affection of the sympathetic. Pregnancy favours the development of purpura, and pregnancy also makes the prognosis distinctly graver, while purpura has nearly always led to the interruption of the gestation. The transmission of purpura from the mother to the fœtus is possible but very rare, and Diehl thinks that his observation is the second well-authenticated one on record, the first being a case described by Dohrn in 1871. The rarity of fœtal purpura is perhaps to be explained by fœtal death or abortion occurring before the disease has time to show itself.

Albuminuria in Pregnancy. — While purpura may be regarded as one of the rarest complications of pregnancy, albuminuria must be looked upon as one of the most frequent. Notwithstanding its frequency there are several matters touching upon the albuminuria of pregnancy which are far from well understood. For instance, the significance of the milder cases is not yet established. Vallois (*L'Obstétrique*, No. 6., p. 585, 1899) has reported two cases of albuminuria in pregnancy in which fœtal death and premature expulsion occurred, and yet in neither instance was the albuminuria more than slight and in both it was transitory. Further, in both cases it was treated by absolute milk diet for the term of the persistence of the albumen in the urine. In one of them, labour came on at the seventh month, and in the other a fortnight later, and in both the puerperium progressed normally. In both the placenta was diseased — numerous old hæmorrhages in one, an enormous number of hæmorrhages of all sizes, colours, and consistences in the other. The conclusion that Vallois draws is that the albuminuria, although slight and passing, and although treated apparently successfully, had produced lesions in the placenta which in their turn had killed the fœtus; he notes, further, that placental lesions may exist when the mother's general health remains good, and in the absence of other serious complications; and he advises that the milk diet be made absolute and continuous. It does not seem to have occurred to him that the placental lesions may have preceded the albuminuria, and that the fœtal death may have caused an amelioration in the conditions and normal puerperia. Nevertheless, it is a well-ascertained fact that the symptoms associated with albuminuria in pregnancy have been found to disappear after the fœtus has died, and before the mother has

been confined. In this relation, a communication by P. C. T. Van der Hoeven (*L'Obstétrique*, No. 5, p. 454, 1899) on the fœtal theory of the production of eclampsia is of considerable interest. He argues that the organs of excretion in such cases are unaltered at the beginning; but the amount of work they have to do is increased for the quantity of toxines to be eliminated is increased during pregnancy. The products of nutrition are poisonous to the organism, and there is no reason for supposing that the products of fœtal nutrition are not toxic to the maternal organism. Both through the placenta and through the liquor amnii toxines pass from the fœtus to the mother, and experiments seem to show that they can pass more quickly by the latter route than by the former. The mother and fœtus both produce toxines, but the mother alone can eliminate them. The physiological toxæmia thus produced may lead to organic lesions, and these to a pathological degree of toxæmia.

DISEASES OF THE EAR, THROAT, AND NOSE

By R. M'KENZIE JOHNSTON, M.D., F.R.C.S.Ed.,

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The Indications for opening the Mastoid in Chronic Suppuration of the Middle Ear.—The great importance of this subject is shown by its selection for an organised discussion at the sixth International Otological Congress, held last summer in London. The discussion was opened by Professor Politzer (Vienna), Professor Macewen (Glasgow), Dr Luc (Paris), and Professor Knapp (New York), and continued by many other speakers.

As was to be expected there was pretty general agreement on the part of the speakers as to the indications for operation in well-marked cases, either to give vent to pus, where there was retention of pus, or when there were urgent symptoms pointing to dangerous complications, or risks of intracranial infection. The chief interest centred, however, in the consideration of whether it was justifiable to operate for the cure of a chronic discharge in the absence of other well-marked or urgent symptoms. In the course of an interesting paper, Politzer expressed his views on this subject, which evidently agreed with the ideas of the majority of his audience. While he is a strong advocate of the radical operation in suitable cases, he does not agree with those who perform it, often for the sole

purpose of curing the discharge—at least until strenuous efforts have been made to cure it by other means. He thought, in these cases, it was not justifiable to have recourse to an operation which, although not dangerous in the hands of a skilled operator, was at least a serious one, especially when they considered: (1) the many important structures in the vicinity which might be injured; (2) the possible impairment of hearing in those who before operation could hear fairly well; (3) the protracted period of the healing process after the operation.

Macewen also contributed a most interesting paper in which he suggested the following practical rule:—When a pyogenic lesion exists in the middle ear, or in its adnexa, which is either not accessible or which cannot be effectually eradicated through the external ear, the mastoid antrum and cells ought to be opened. While distinctly disassociating himself from the view that the operation should be performed for the cure of discharge alone, he evidently believes that early operation in many cases tends greatly to the safety of the patient.

Luc, in his valuable contribution, holds that operation for the cure of otorrhœa is indicated after it has been recognised as intractable to different methods of local treatment applied through the meatus, including the extraction of the ossicles and the curetting of granulations through this passage.

Knapp does not place much reliance in the permanence of cure from intra-tympanic operations, but looks upon the opening of the mastoid in chronic discharge both as a curative and prophylactic measure.

The report of the discussion is most interesting and is well worth perusal, even though it does not produce any short-cut to help us to decide when these border-line cases should be operated on. Each must still be considered upon its own merits. The great danger of allowing a discharge to continue unchecked, and the necessity for perseverance in treating it by every means at our disposal was very fully emphasised. It seems manifest that a number of cases of apparently intractable discharge can be successfully cured by energetic and appropriate treatment without opening up the middle ear. At the same time, without siding with the operative enthusiast, it is evident that there are also many cases that might be operated on with advantage without waiting for the onset of urgent symptoms. (*Journal of Laryngology*, October 1899.)

A new symptom of Obstructive Thrombosis of the Lateral Sinus.—According to the symptoms mentioned by Leutert, it may require several days' observation to establish a diagnosis of sinus thrombosis. Voss of Riga points out that this time may be

considerably shortened by the assistance of the stethoscope. If this instrument be placed over a normal vessel, as for instance the jugular vein, a murmur can be heard which will not be the case if the vessel be obstructed. If, however, the murmur is not heard, slight pressure with the stethoscope will induce it in the healthy vessel, but will not do so if the vessel be obstructed. The author therefore considers that a continuous hæmic murmur in the jugular under pressure of the stethoscope indicates that the vessel is normal, and its absence is characteristic of sinus thrombosis. The best position to apply the test is close to the base of the skull. He has been able to demonstrate the accuracy of this observation upon several occasions. (*Archiv. of Otol.*, Vol. xxviii., No. 4.)

The Supra-renal Bodies: with special reference to Rhinology.—In a paper by Newcomb, which is chiefly devoted to a review of the physiology and therapy of the glands, he refers to their local use in nose and throat work. An aqueous extract of the gland can be used in considerable quantity without local or constitutional ill effects. It is a most powerful vaso-constrictor and contractor of erectile tissue, and this effect can be produced repeatedly in the same individual without harm. It can, therefore, be of great service in rendering minor operations on the nose practically bloodless. It is of special value in alleviating the congestive conditions of the nose, which are so often associated with the hay-fever type of disease.

The aqueous solution tends to decompose very rapidly, and the addition of an antiseptic appears to destroy its activity. In order to overcome this difficulty Lederman adds pure glycerine to the solution, and finds that this does not interfere with the physiological action of the drug, and enables it to be kept for some little time.

To prepare the solution he takes ten grains of the dry gland extract and a drachm of water containing 25 per cent. of glycerine. The mixture, placed in a bottle, is well shaken and allowed to stand for about forty-eight hours, during which time it is occasionally shaken. It is now filtered through blotting-paper and bottled ready for use. It may be mentioned that it has no anæsthetic action, so that cocaine or eucaine must also be used if any painful procedure is contemplated, but these drugs must be used separately and not mixed with it. (*The Laryngoscope*, Vol. vi., No. 1 and 6.)

On the Intestinal Disturbances produced by Otitis Media of Infants.—Hartmann has undertaken observations, along with Kossel, which tend to show:—

1. Otitis media of infants may be accompanied by disturbances of nutrition, as evidenced by dyspepsia and emaciation.
2. After paracentesis, the disturbances in digestion disappear, and weight increases.
3. Elevations of temperature in the course of intestinal disturbances may be due to an otitis.
4. In all intestinal affections of infants, accompanied by elevation of temperature and reduction in weight, an examination of the ears for possible presence of otitis should not be neglected. (*Archiv. of Otol.*, Vol. xxviii., No. 2 and 3.)

Note

ANTENATAL PATHOLOGY AND TERATOLOGY.—*University of Edinburgh.*—J. W. Ballantyne, M.D., F.R.C P.E., F.R.S.Ed., University Lecturer, will deliver four lectures on this subject in the Medical Jurisprudence Class-room, University New Buildings, on Fridays, February 2, 9, 16 and 23, at 5 P.M. These lectures are free to members of the University and practitioners of medicine.

Books Received

A SYSTEM OF DISEASES OF THE EYE. By American, British, Dutch, French, German and Spanish Authors. Edited by W. F. Norris, M.D., and Charles A. Oliver, M.D. Vol. IV. London and Philadelphia: J. B. Lippincott Company. 1900. Price 21s.

ENCYCLOPÆDIA MEDICA. Under the General Editorship of Chalmers Watson, M.B., M.R.C.P.E. Vol. II. Edinburgh: William Green and Sons. 1899.

A SYSTEM OF MEDICINE. By many writers. Edited by Thomas Clifford Allbutt, M.D., LL.D., F.R.S. Vol. VIII. London: Macmillan and Company. 1899. Price 25s.

LA TUBERCULOSE EST CURABLE. By Dr Elisée Ribard. Paris: Georges Carré et Naud. Price 2 frs.

HEALTH ABROAD. A Medical Handbook of Travel. By various authors. Edited by Edmund Hobhouse, M.D. London: Smith, Elder and Company. 1899. Price 6s.

HEART DISEASE WITH SPECIAL REFERENCE TO PROGNOSIS AND TREATMENT. By Sir William H. Broadbent, Bart., M.D., F.R.S., and John F. H. Broadbent, M.D. Third Edition. London: Ballière, Tindall and Cox. 1900. Price 12s. 6d.

DIFFICULT DIGESTION DUE TO DISPLACEMENTS. By A. Symons Eccles, M.B. London: Ballière, Tindall and Cox. 1899. Price 4s.

DISEASES OF THE NERVOUS SYSTEM. By H. Campbell Thomson, M.D. London: Ballière, Tindall & Cox. 1899. Price 4s.

DICTIONNAIRE DES TERMES DE MÉDECINE, Français-Anglais. London: Ballière, Tindall & Cox. 1899. Price 4s.

A MANUAL OF SURGERY. By C. Stonham, F.R.C.S.Eng. Vol. III. London: Macmillan & Company, Ltd. 1899. Price 10s. 6d.

ON THE PREVENTION OF EYE ACCIDENTS OCCURRING IN TRADES. By Simeon Snell, F.R.C.S.Edin. London: Bale, Sons & Daniellsson, Ltd. 1899. Price 1s.

LETTER- WORD- AND MIND-BLINDNESS. By James Hinshelwood, M.A., M.D. London: H. K. Lewis. 1900. Price 3s.

A MANUAL OF DISEASES OF THE NOSE AND THROAT. By Cornelius Godfrey Cookley, M.D. London: Henry Kimpton. Price 12s.

LESSONS ON PRESCRIPTIONS AND PRESCRIBING. By W. Handsel Griffiths, Ph.D., L.R.C.P.E. Second Edition. London: Macmillan & Company, Ltd. 1899. Price 3s. 6d.

AIDS TO MATERIA MEDICA. By William Murrell, M.D. Part III. London: Ballière, Tindall & Cox. 1900. Price 2s. 6d.

NOTICE.

In future all communications to the Editorial Department, including Exchanges and Books for Review, should be addressed to THE EDITOR, Dr W. RUSSELL, 3 *Walker Street, Edinburgh; and all business communications, including Subscriptions, Advertisements, &c., to* THE SUB-EDITOR, Dr NORMAN WALKER, 7 *Manor Place, Edinburgh.*

THE SCOTTISH
MEDICAL AND SURGICAL
JOURNAL

CLINICAL LECTURE

SOME ILLUSTRATIONS OF THE CLINICAL SIGNIFICANCE AND IMPORTANCE OF DECUBITUS

Delivered to the Class of Clinical Medicine in the Royal Infirmary, Edinburgh, on 16th January 1900

By J. O. AFFLECK, M.D., F.R.C.P.Ed.,
Physician to the Royal Infirmary; Lecturer on the Practice of Medicine, and on Clinical Medicine, Royal Colleges School of Medicine, Edinburgh

THERE can be little doubt that while clinical medicine owes much to means of diagnosis which modern science has put in our power, it owes still more to the faithful and accurate observation and recognition of the phenomena of disease as manifested by the living sufferer. From the very earliest times in the history of our profession there have always been earnest men engaged in watching the external evidences of disease and recording their observations, and so it has come to pass that we possess a body of facts daily proved to be reliable and of the utmost value from a diagnostic point of view, many of which date back to the very infancy of medicine. That the field is by no means exhausted I feel very certain, and it is in the hope of interesting you in it, and enlisting you as workers, that I venture to bring under your notice some facts from one department of this domain of observation in disease.

Some might fancy that the subject is one fully better suited for a post-graduate class; but I hold, on the contrary, that it is eminently fitted for undergraduates, since assuredly in this field of work it is impossible to begin too early.

I have selected the subject of Decubitus or Posture in dis-

ease, and I propose to give some illustrations with the view of showing the value of this clinical evidence in diagnosis, in prognosis, and even in treatment.

We are all of us familiar with the fact, that in health there are many interesting points capable of being observed in individuals concerning attitude, posture, gait, etc., which often prove of great value in enabling us to trace resemblances among persons related to each other where even the features are no guide. Again, there are sometimes to be noticed peculiarities or idiosyncrasies in individuals, which, however curious, and it may be inexplicable, are yet consistent with perfect health. Thus one person may be unable to sleep unless his head is high; another unless it is low, and a third can only rest comfortably on one side. It is not of such instances that I propose to speak, but rather of decubitus or posture as an *evidence or accompaniment of disease*, and I hope to be able to show that information of a valuable kind may often be derived from observing a patient's method of lying, or disposing his body or limbs in various morbid states.

If you walk round any of our large medical wards you will scarcely fail to see some patient lying in a certain posture, which he will tell you is the only one available for him, and to attempt to change which would give rise to such suffering as would clearly show that none other is possible for him.

In the familiar disease, *Rheumatic Fever*, you must have noticed how extremely helpless the patient appears as he lies on his back as if fastened or soldered to the bed, almost the only part of him he dare move being his eyes; while he dreads the slightest motion of his limbs, body, or head. Again, it is an observation almost as old as medicine itself, that *in a fever* it is a good sign to find the patient *lying on his side*, particularly if that attitude is assumed spontaneously, testifying as it does to a fair measure of muscular strength; while, on the contrary, when the patient *lies on his back*, and tends to *sink down* low in the bed, we regard this as an evidence of great debility, and often of evil omen. One serious accompaniment and result of this latter posture is the condition known as hypostatic congestion of the lungs, which is apt to become a formidable complication, embarrassing the breathing, and often determining a fatal issue. Taking the hint from

this, it is often excellent practice in attending a case of long lasting fever or debilitating disease, to have the patient occasionally moved round on one or other side, so as to prevent blood stasis in the posterior parts of the lungs.

Passing from these general examples, and with the object of giving point to my illustrations, I propose to select instances from diseases affecting the more important systems of the body, thus:—

1. *Illustrations of Decubitus from Diseases of the Respiratory System*

In *Acute Pneumonia* the decubitus is interesting, and is well exemplified in the case of a patient who came into my ward yesterday. I found the man lying upon his right side (it proved to be a case of pneumonia of the right base), and on asking him if he was easier in that position, he at once replied, "I lay first best on my left side, then on my back, and now on my right side." The reason is tolerably clear. At the commencement of pneumonia the patient suffers from sharp pain in the affected side, due to the accompanying pleurisy, and he avoids lying upon that side. This pain tends, however, to lessen, and he gradually moves round upon his back, and subsequently upon the affected side in consequence of the greater freedom in breathing which these positions afford him. I do not altogether like a case of pneumonia where the decubitus is entirely dorsal, and sometimes I have the patient turned a little round upon the affected side if he can bear it.

To a considerable extent the same points hold good in *Pleurisy with effusion.*

In *Bronchitis* the patient often lies with the head elevated, and sometimes the position is that of orthopnœa. It is usually a good sign of the cessation of an attack when the head can be borne lower. But not always, for sometimes you may find in the feeble and aged the patient sinks gradually down, becoming apathetic and cyanotic, death soon supervening.

In *Asthma* the patient generally selects the position and attitude easiest for himself, sometimes in bed, sometimes out of bed; and any attempt to interfere with this only aggravates his distress.

In *Phthisis* the decubitus varies often according to the stage, or to the presence of complications such as pleurisy. In the early stage it frequently happens that lying on one side aggravates the cough much more than lying on the other, and it may even in some cases tend to bring on hæmoptysis. In the later stages when there is excavation you may often, although by no means always, find the patient lying on the same side as that where the cavity exists, since if he turns to the less affected side the secretion is poured out from the cavity into the bronchi and trachea and sets up coughing. The fortunately rare occurrence of *Pneumothorax* if it takes place suddenly causes orthopnœa.

In cases involving direct pressure on the trachea or bronchi, such as is illustrated in the person of a young woman at present in Ward 28 with a rapidly growing sarcoma in her anterior mediastinum, the posture may be (as in this case) sitting up and leaning forward so as to allow the tumour to fall away from the air passages as far as may be. In such instances where this position had to be continued night and day for weeks together, I have seen great œdema of the lower extremities produced.

In obstructive conditions within the larynx as seen in laryngitis or diphtheria, the retraction of the head is often very noticeable.

2. *Illustrations of Decubitus from Diseases of the Circulatory System*

In *Cardiac Disease*, especially those forms involving enlargement of the heart, the patient generally lies on the right side. When the case is one accompanied with dropsy this may sometimes give rise to a somewhat striking appearance, namely that of unilateral anasarca ; and so also to a unilateral œdema of the lung and hydrothorax. Cardiac dropsy usually begins, as you know, at the feet. It is useful at the commencement to get the patient to lie in bed or to have the feet raised and a roller bandage applied. In the later stages of cardiac dropsy where the whole body is water-logged, such as in advanced mitral disease, the patient may be unable not merely to lie down but even to remain in bed, and passes his days and

nights sitting in a chair owing to the suffocative feeling which any attempt to assume the recumbent position entails. But not alone in dropsical conditions may this be the case, but also in certain forms of heart trouble unattended with any such complication, as in very feeble or fatty heart. That the feeling, in such cases, is apparently sometimes largely due to nervous perturbation may frequently be shown by the fact that when a sufficiently strong sedative is administered, such as a good dose of paraldehyde, the patient may be quite able to lie down and to breathe easily in that position.

In *Pericarditis*, especially where any amount of effusion is present, the patient's most comfortable position is that with the head and shoulders raised, and sometimes even sitting erect in bed (orthopnœa).

In *Angina Pectoris* the patient keeps his body as still as possible, from the feeling that any effort may precipitate his end. But this is not always the case, as I have actually seen the sufferer pacing up and down the room in the agony of a paroxysm just prior to the fatal collapse.

In *Aneurism* the patient assumes the position giving greatest ease in breathing or exemption from pain. Where the aneurism presses against the trachea, the decubitus is similar to that described as occurring in tumours of the anterior mediastinum. On the other hand where the aneurism is pressing against ribs or sternum or vertebræ, the decubitus may be wholly dorsal or lateral.

3. *Illustrations from Diseases of the Alimentary System*

In *Gastric Ulcer* the patient as a rule lies on the back, but where there is much pain it may be only possible to lie on one or other side, or as I have occasionally seen, on the face. In many instances the upright position and the pressure of the clothing over the epigastrium are ill borne, and the mere laying the patient in bed is sufficient to bring marked relief. Let me here say that I do not think either the position of the pain or the decubitus is necessarily any guide to the site of the ulcer in the stomach. No pain is more apt to be reflected (that is, to be felt at some distance from the part) than that of gastric ulcer, and the decubitus may be entirely

independent of the position of the disease, so that it would not follow that because the patient lay upon the face the ulcer was in the posterior wall of the stomach. Nevertheless the attitude in which the patient finds greatest ease is always worth being noted.

In *Diseases of the Liver*, especially those producing enlargement such as cancer, the patient as a rule lies upon the right side and cannot turn on the left without experiencing a sense of dragging or oppression, probably from the falling over of the enlarged organ upon the heart.

In *Acute Peritonitis* the attitude in bed is very characteristic. The patient lies on his back with his knees drawn up, and the breathing is mostly thoracic. These two points are highly diagnostic. In *Appendicitis* sometimes the right leg only is drawn up.

In *Ascites*, when the quantity of fluid is large, the patient's easiest position is lying on the back with the shoulders raised. The pressure of the fluid upward upon the diaphragm impairs the breathing and contributes to produce pulmonary œdema and hydrothorax, and this is a clear indication for the performance of *paracentesis abdominis*.

4. *Illustrations from Diseases of the Nervous System*

In *Tubercular Meningitis* the position of the body and head are often very informing. At first the patient lies very much in one position, either on the side or back, and seems fixed to the bed and pillow. Attempts to change the position or to lift the head cause the child to scream. Later on the patient lies on the side with the knees drawn up, the abdomen hollow and the head retracted. Should the tubercular condition affect, as it sometimes does, the spinal cord also, there may be considerable arching of the back.

In *Apoplexy* the decubitus at the outset is often dorsal, with the head and eyes markedly deviated to the one side (usually the side of the cerebral lesion and opposite to the paralysed side). The patient's face is frequently flushed or dusky. It is a good thing to raise the head slightly. When there is hemiplegia the patient often lies with the paralysed arm at right angles across the body and the leg stretched

out. Unless some attempt is made from time to time to change slightly the position of these paralysed members, they may become very painful.

In *Intra-cranial Tumours* the patient may sometimes find relief to the headache from lying in one particular position. There is at present in Ward 28 a woman who shows the symptoms of a cerebral tumour involving the left motor area, and who finds ease from her agonising headache by lying on her right side with her head firmly applied to the pillow. The accompanying giddiness is sometimes relieved in the same manner.

The mention of giddiness here reminds me of some cases which I have seen mostly in elderly people, where lying upon one side produced a feeling of the most violent vertigo, which was at once dispelled by turning on the other side. Such cases showed no evidence of any organic brain disease, and no explanation seemed attainable beyond the not very satisfactory one of some sudden ischœmia of the cerebral circulation. I have found large doses of bromide of sodium relieve the symptoms very effectually.

In *Myelitis*, where the decubitus is usually dorsal, the lowered vitality of the skin predisposes to a bed-sore on the back, which may take place notwithstanding everything that can possibly be done to avert it. It is not enough that the patient should lie upon a water bed. It is also necessary that the surface upon which he lies should be absolutely smooth, any fold in the sheet or irregularity being often sufficient to start an abrasion of the skin, which soon becomes converted into a bed-sore by the irritating effects of the urine which is passed in bed. In like manner care has to be taken against applying hot bottles directly to the paralysed limbs, the skin being vulnerable and easily burnt unconsciously to the patient.

In *Spastic Paraplegia* the rigidity of the muscles of the lower extremities may cause locking of the legs in bed. A condition similar to this may sometimes be seen in hysteria. Indeed the possibility of the last-named affection is never to be excluded in the investigation of paralytic symptoms presenting themselves in women.

In *Alcoholic Neuritis*, the dorsal decubitus with more or less complete paraplegia and marked foot-drop is frequently

observed. In the absence of a history of the case (which is sometimes difficult to obtain), the wasting of the limbs, the cutaneous sensory dysæsthesiæ or anæsthesia, and the marked muscular hyperæsthesia, taken along with the mental impairment especially in the domain of memory, are important aids in establishing a diagnosis. But mistakes in this disease are not uncommon.

In the rare affection termed *Catalepsy* the body remains rigidly fixed in the position in which it was at the time of the seizure, and this may give rise to strange attitudes. But the limbs are capable of being relaxed by others, and when placed in any one position will so remain until they are changed again. As a contrast to this, in the late stages of some nervous diseases such as multiple cerebro-spinal sclerosis and pseudo-hypertrophic paralysis, there is such complete relaxation of the limbs that they may be made to go in any direction.

In *Tetanus* the condition of opisthotonus may be persistent or paroxysmal, more frequently the latter. In a case under my care some time ago, certain of the muscles of the abdominal wall were found torn by the violence of their contraction in the paroxysms.

In illustrating my subject it will be seen that I have only selected a few examples of decubitus or posture from the diseases of the various systems referred to. They are all such as have come under my own observation, and they might easily have been multiplied had time permitted. It must not be supposed, however, that the instances now cited represent conditions of constant occurrence. On the contrary, frequent exceptions may be met with.

As mentioned at the outset, my object in this lecture has been less to state inflexible facts than to use these illustrations as means of encouraging and stimulating you in the practice of careful observation in the prosecution of your clinical work, and I shall be gratified indeed if they may prove in any way helpful to you in this direction.

ON THE CAUSE OF THE DIFFERENTIATION OF CONNECTIVE TISSUE IN THE HUMAN FŒTUS

With Special Reference to the Essential Nature of the Hydatid Mole

By D. BERRY HART, M.D., F.R.C.P.Ed.,

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WHEN the embryologist has settled the by no means easy question of the actual moulding of an organ or tissue, he has still many difficult problems to face, and has in fact only touched the fringe of his subject. Before him lies, for instance, that interesting question of the differentiation of cells to form tissues. Why should certain of the cells of the ectodermic layer form the nervous system, while others of the same origin give rise to skin, Wolffian duct, or sweat gland? To such a question, as yet, we have no answer, although there seems to me one general fact of interest, viz., that an important and delicate tissue originating from ectoderm, say, does so early in development, while less active organs arise later, *i.e.* further removed, in point of time, from the original dividing fertilised cell or oosperm. The nervous system originates soon, the Wolffian duct later, the sweat glands later, and so on. I could multiply instances of this, but pass on to consider the phase of differentiation of tissue concerning us at present and the cause of it.

For some time past, in studying embryological questions, I have been struck with some curious and apparently anomalous facts in the origin, development, differentiation, and pathological conditions of the connective tissue of the human embryo. The connective tissue of the human fœtus passes through various stages before it becomes of a fully formed type. Thus, in early embryos, 3-4 mm. in length, it consists only of round embryonic cells, which later on, say at the end of the first month, and very markedly during the second and third, form a network with spaces and a mucin-containing fluid in them. Ultimately in the fœtus the myxomatous tissue, except in a few localities, passes on to a fully formed

areolar type with its increased fibrillation, spindle cells, and relative deficiency of mucin-containing ground substance.

I do not, of course, at present consider all the varieties of fully-formed connective tissue but take up merely the early embryonic stage, the myxomatous stage or variety, the stage or variety represented in the villi, and the fully formed areolar type, or ordinary connective tissue.

All these stages can be well seen in the following specimens:—the earliest stage in a 3-4 mm. human embryo; the myxomatous variety in the umbilical cord and in the pelvis, or in fact any part of the body of a two and a half month fœtus; the stage intermediate between this and the fully formed type in the placental villi; and the fully formed type in the seven months' fœtus, perhaps earlier; it is still of a myxomatous type at the fourth and a half month.

I must now detail the condition of the connective tissue at these various stages.

In the *early embryo*, 3-4 *mm.*, we have corpuscles without processes and a ground substance with some fibrillation.

In the amnion of this specimen the connective tissue is markedly myxomatous and not quite so myxomatous in the villi and chorion.

In the umbilical cord of the two and a half months' fœtus the characteristic structure of this tissue is seen to perfection. There are branching corpuscles with a ground substance of fibres, spaces containing mucin, and a concentric arrangement round the vessels.

The connective tissue in the pelvis is of a myxomatous type but somewhat less marked, *i.e.* the corpuscles are more elongated and with a much less evident triradiate arrangement of fibres.

I do not know exactly as yet when the myxomatous tissue ceases in the fœtus. I have found it at the fourth and fifth month, but not at the seventh.

The *epidermis* at the second and a half month has a triple arrangement of cells, the upper being at places dome-shaped as Minot points out. The cord epithelium is double-layered at most parts. In the villi of the early placenta the tissue is myxomatous. At the fourth and a half month it is much more fibrous, and at the full term distinctly so.

In the adult the connective tissue is all of a fully formed type, with the exception of the vitreous humour, where it is of a myxomatous type, mainly ground substance with few fibres or cells. In the dental pulp it is also somewhat myxomatous.

A very remarkable fact has now to be noted, viz., that in certain parts of the fully formed fœtus and its appendages, what we have described as early stages in the development of connective tissue forms the permanent condition for that organ or site. Thus, the tissue of the umbilical cord does not advance beyond the myxomatous stage; this is the fullest development. The tissue in the core of the early villi is markedly myxomatous, but in fully formed placenta approximates more to the adult type of connective tissues.

What I have brought forward as to the development of the connective tissue holds good for the development of epidermis. It begins as a single layer, becomes multiple, shows stratification, and finally passes on to fully formed skin. Each of these stages is permanent for some tissue. The single layer is permanent for the amnion, the double for the chorion, the stratified for the cord, and the fully formed for the fœtal skin.

We have now to consider if we have any key or explanation to offer for the facts I have just summarised.

This came to me from a side issue. I happened to come across the statement in "Schäfer's Physiology" that Sir F. Semon had suggested, in regard to myxœdema of the adult, "that removal of the thyroid produced an interference with the full chemical development of the constituents of the connective tissues, so that these tend to take on an embryonic character" (Schäfer, in "Schäfer's Physiology," foot-note, p. 942, and also *Brit. Med. Journal*, 1883). On looking up other authorities I found that Bramwell discusses, in his work on "Anæmia," two views: one that the thyroid secretes a substance or substances necessary "for the healthy and satisfactory nutrition of the tissues, more especially of cerebral and nervous tissues" ("Anæmia and Diseases of the Blood-forming and Ductless Organs," p. 297), and a second, that it separates from the tissues some poisonous substance which in atrophy of the thyroid has full sway in the body and acts poisonously. He rightly holds the first view. R. Hutchison also discusses the

matter ably. It occurred to me that one might modify or adapt Semon's theory for the human embryo as follows:—In the fœtus does the thyroid secretion among other functions not favour or cause the development of connective tissue from the myxomatous to the fully developed areolar form? I merely use this as a working theory, but it leads to some interesting and important results.

The thyroid in the fœtus develops at an early period, *i.e.* in embryos of 5 mm., and therefore about the end of the third week. It is well developed in the fœtus and there is no doubt as to its activity. In a section of the thyroid of a two and a half months' fœtus one can note its great vascularity and the presence of colloid, which is the active element, in the lumina of the glands. An interesting functional proof of its activity is given in the well-known Report on Myxœdema of the Clinical Society, where a case is recorded of temporary cure of myxœdema in the mother during pregnancy, while in another case improvement was also noted during pregnancy (cases 9 and 85 in the Report). I have little doubt Dr H. W. G. Mackenzie's suggestion is the right one, viz., that the improvement in the mother was due to the absorption of the fœtal thyroid secretion. [In case 9 of the Clinical Society's Report on Myxœdema the note is "Pregnancy after leaving hospital, a child, healthy, full term; marked, but temporary, diminution of myxœdematous condition" (observer's name, Cavafy). In case 85 "Disease is said to have disappeared completely on two occasions during pregnancy, and to have returned after confinement" (observer's name, Ord).]

As is well known the secretion of the thyroid is an internal one, *i.e.* is formed in a ductless gland, absorbed by its blood-vessels, and possibly by its lymphatics. It next enters the venous circulation, is carried through the arterial system, and distributed to the connective tissue through the lymphatics and capillaries. Blood and lymphatic circulation, *i.e.* capillary blood-vessels, lymph spaces, and lymphatic vessels in a tissue are apparently necessary for its full thyroidisation.

Now comes an important series of anatomical facts. The cord of the fœtus where the connective tissue remains embryonic, is not vascularised either by blood or lymph vessels, and has not even lymph spaces, so far as is known. The umbilical

arteries give off no capillaries in it, and thus the umbilical cord is not at any time thyroidised—it therefore remains an embryonic tissue with a covering short of fully formed skin. It looks as if its nourishment were due to actual transmission through the substance in the cord itself. The same holds good for the amnion. On the other hand, the tissues of the villi and chorion have no lymphatics, but are vascularised, are in relation to maternal tissue, and with this as a source for thyroid we may associate their higher type of connective tissue and epithelium. In the fœtus proper we have an abundant vascular and lymphatic supply, and therefore full development of skin and connective tissue, except in the vitreous humour and dental pulp. In the vitreous we have no vessels, but lymphatics exist; while in the dental pulp, to which Dr Noel Paton directed my attention, we have blood-vessels, but no lymphatics. They thus tend to approximate more to a myxomatous type than a fully formed one. The dental pulp, as figured by Röse, is distinctly fibrillated. I hold, therefore, that for the full thyroidisation of a tissue we require blood-vessels and lymphatics as well as a certain element of time in the fœtus—here measured by months. Connective tissues not fully developed have some modification of vessels, *i.e.* either have no vessels or no lymphatics, although, of course, they have lymph spaces.

My view as to the effect of the thyroid on the fœtal connective tissue may now be put thus:

(*a*) Fœtal connective tissue unprovided with vessels, lymphatics, and lymph spaces, remains permanently myxomatous, as in the case of the umbilical cord.

(*b*) Connective tissue with only a vascular supply and no lymphatics (dental pulp) falls short of the fully formed type. The villi, it must be remembered, share in the maternal thyroid secretion as well as in the fœtal, although the latter is less perfect.

(*c*) Tissues with a lymphatic but no vascular supply also approximate to the myxomatous type (vitreous humour and cornea).

The fœtal tissues, as it were, show us the relation of the thyroid secretion to tissues with various grades of vascular and lymphatic supply. Thus we get tissues with neither lymphatic

nor vascular supply; one with vascular and no lymphatic supply; and two with lymphatic but no vascular supply. In this way the fœtus experiments therapeutically with various grades of absorption of the thyroid secretion.

A striking confirmation of the feasibility of this working theory is to be found in the hydatid mole. In this comparatively common form of mole we have a marked change in the chorion, a conversion of its whole structure into a mass of muciparous tissue, which we generally describe as the myxomatous degeneration of the chorionic villi. As a rule no fœtus is to be found, although Dr Allan Sym has recorded a case of partial myxomatous degeneration of the placenta with the fœtus present, and prematurely born at the seventh month. The fœtus was not specially examined, and the whole specimen was unfortunately accidentally destroyed some time afterwards. I think, however, that valuable information will be got from the careful examination of the thyroid of such specimens. The point as to whether a twin was not present should be considered.

The nature of the hydatid mole is quite unknown, but based on what I have already suggested, it seems to me most likely to be an acute myxœdema of the chorion, the fœtus having ceased to develop at a very early period and prior to the development of the thyroid. Myxœdema in the adult exists usually in a chronic form, but probably Dr Ord's case, where the mucin was estimated by Dr Charles, was an acute form, and thus an excessive quantity of mucin was found in the subcutaneous and other tissues.

In the hydatid mole, analysis has shown an increase in mucin, but fresh work on this point will be necessary as specimens come to hand. I urge, therefore, and repeat this view of the hydatid mole as a probable one. There is non-development of the fœtus before the thyroid forms, *i.e.* prior to the third week. The chorion itself lives and continues developing its myxomatous elements, mainly villous. The villi normally have a myxomatous core up to beyond the second month and probably into the third, and as a rule this is the time for such moles to develop. Too little attention, however, has been paid to the core of the villi, and almost all controversy has raged over their epithelial covering. It will be important to trace

more exactly at what age of the placenta the myxomatous core begins to become more fibrous. I may note here that the villi in the moles I have examined do not contain any fœtal vessels as in the normal villi. If a hydatid mole be myxœdema of the chorion, it is an acute and not a chronic form and the reverse of the adult as to frequency.

I have, however, recorded a case of fibrous mole, *i.e.* of a hydatid mole retained for nine months, and in this the myxomatous villi had become more fibrous. There was probably, during the retention, thyroidisation from the maternal side, and thus a higher stage than myxomatous tissue obtained. This case would correspond more nearly to the adult myxœdema.

It might be urged against this view of the hydatid mole that a mother with myxœdema should, when conception takes place, have a hydatid mole. The fœtus in myxœdematous mothers has, however, its thyroid and develops normally.

In regard to sporadic cretins, it is to be noticed that in most cases the cretinism develops after birth; in the fœtus the thyroid is present.

Congenital cretins may have had thyroid disease late in fœtal life. For the production of the hydatid mole my view requires non-development of the embryo or absence of its thyroid at the third to fourth week.

The question may now be asked if the view advanced has any therapeutical bearing. There are evident reasons against the probability of one's preventing hydatid mole by administering thyroid extract, as diagnosis early enough is impossible. I think, however, that thyroid extract should be given after hydatid mole abortion. The question arises as to the relation of the thyroid to deciduoma malignum which follows, with some frequency, hydatid mole. In no case of deciduoma malignum have I been able to find any note as to the condition of the thyroid; but it seems to me that in future in such cases the condition of this gland should be investigated.

Before summing up my conclusions on this matter, I may point out that the connection between defective thyroid action and adult myxœdema is absolutely proved by pathological, therapeutic and experimental evidence.

In the case of the fœtus and the hydatid mole the evidence of thyroid action and of defective thyroid action is not as

yet so complete, and although I have endeavoured not to overstrain it, I wish to put forward my opinions with this reservation.

I may now sum up as follows :—

1. There is anatomical and clinical, but not as yet any experimental, evidence that the thyroid is functionally active in the embryo.

2. The thyroid begins to develop in fœtus of 5 mm., *i.e.* about the twenty-first day, and is well developed as the fœtus grows.

3. It probably causes the development of connective tissue, when both lymphatics and blood-vessels are present, to pass from the myxomatous to a higher stage, the areolar. Thus :—

(1) The cord, without vessels and lymphatics, and presumably without thyroidisation, remains myxomatous.

(2) The adult vitreous, with only lymphatics, remains more allied to myxomatous tissue than any higher form ; the same holds good for the dental pulp which is provided with blood-vessels only.

(3) Tissues like the villi are more or less myxomatous according to their age and length of exposure to vascularisation.

4. The free covering of somatopleure, *i.e.* skin, cord, amnion, chorion, is also probably affected by the thyroid.

5. The hydatid mole may be regarded as acute myxœdema of the chorion.

6. Thyroid extract should be administered after hydatid mole abortion, to favour the passage of any myxomatous villi left, to a fibrous state.

The relation of the thyroid to cases of deciduoma malignum has yet to be investigated.

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CASE OF A PECULIAR FORM OF DWARFED GROWTH

By JOHN THOMSON, M.D., F.R.C.P.Ed.

With Notes of the *Post-mortem* Examination by
JESSIE M. MACGREGOR, M.D., L.R.C.P. & S.Ed.

A LITTLE girl, aged four years and eight months, was brought to the Sick Children's Hospital Dispensary on February 26th, 1897, with the complaint that she was not growing and took "fits" from time to time.

Family History.—Her parents were healthy and respectable people. Her mother had had ten children, of whom the patient was the eighth. Of the other children, four had died from acute illnesses. One of these, the fourth in order, was said never to have grown properly and to have been always dull. The tenth child was a well-marked sporadic cretin. The others were all well-grown and normal. Neither father nor mother knew of any idiots or dwarfs among their relatives. During her pregnancy the mother never felt quite well, but she suffered from no actual illness or accident.

Personal History.—At birth, the child was regarded as a big baby, and she was very fat. She was on the breast for eleven months and seemed to thrive well. Before she was six months old, however, her mother noticed that she was smaller than she should be. In other respects she seemed normal. She was very bright, lively, and good-natured; her circulation seemed good and she was never chilly; her bowels moved regularly and naturally. When seven months old she began to get her teeth, and she cut them rapidly and easily.

When nine months old she took her first "turn" or "fit." In this she became unconscious and her face assumed a bluish tint. Her breathing was hurried and her eyes turned up, but her limbs did not move. No cause for this attack was known. Three or four months afterwards she had another similar seizure, and after this they recurred, at irregular intervals, about once a month.

Her mental development proceeded normally, and her mother thought she was quite as intelligent as other children of her age. When about 3½ years old she measured 27 inches in height.

State on Examination.—The patient looked like a child of eighteen months old (Fig. 1). Her height was 28½ in.; her weight 20 lbs. 7 oz. The circumference of the cranium measured 18⅛ in.; that of the thorax 18 in.; that of the abdomen 20 in. The temperature in the groin was 97·4°F.

The body was well nourished and fairly normal in its proportions. The skin was soft and natural; the hair light brown in colour, fine in texture, but rather dry and scanty. The forehead was prominent, the cheeks fat, the bridge of the nose flattened, and there were distinct epicanthic folds. The mouth was small, with thin, firm lips; the tongue normal in size and appearance; the palate not abnormally arched. The ears were well shaped. There was a good deal of fat about the neck, but no distinct fatty tumours. The condition of the thyroid gland could not be made out with certainty.

The abdomen was normal. There was no umbilical hernia, and the liver and spleen were not enlarged. The heart and lungs were also normal.

The hands and feet were small and neatly formed—the fingers being slender and tapering. An X-ray photograph, kindly taken by Dr Rainy, showed that the ossification of the carpus and hand was rather backward and resembled that of a normal child of two or three years. The only nuclei of ossification visible in the carpus were those of the os magnum and unciform. The centre of ossification of the epiphysis of the metacarpal bone of the thumb was just beginning to be visible.

The limbs were firm and fairly muscular, and the child was active and quick in her movements.

FIG. 1.—DR JOHN THOMSON'S CASE OF DWARFED GROWTH.

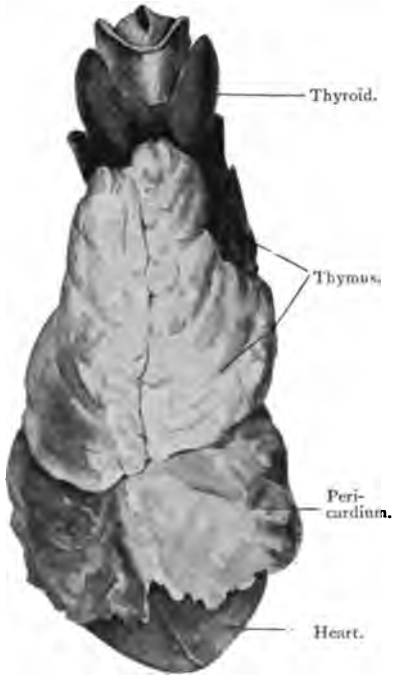

FIG. 2.—SHEWING THE ENLARGED THYMUS.

PLATE IX.—TO ILLUSTRATE DR JOHN THOMSON'S PAPER.

The voice was clear but rather squeaky and high-pitched. The intelligence seemed quite good and the memory particularly so. Careful measurements were taken of the whole body.

Progress.—On March 2nd, 1897, thyrocol[1] (the colloid matter of sheep's thyroid) was begun in doses of ¼ gr. every day. This was increased until, by March 29th, she was having ½ gr. every day. This dose was continued till November without inconveniencing her in any way. In the end of April she took whooping-cough; she had it mildly and made a complete recovery.

On November 4th, her height was 29½ in., the limbs were a little longer than when first measured, but the hands and feet had not grown and the fontanelle had not diminished in size. The dose of thyrocol was increased to gr. i daily.

On January 12th, 1898, the child was found to measure 30⅛ in. in height and to weigh 21 lb. 4 oz. The fontanelle was about two-thirds of its former size. She was rather thinner than she used to be, but her general health was very good and she had had no fits since the beginning of the thyrocol, except one in December. She was ordered grs. ii of thyrocol daily.

On January 20th, this was increased to grs. iii; on January 26th, to grs. iv, and on February 9th, to grs. v in the day. This was continued until March 23rd, after which half of the amount was given. During March the child was occasionally sick in the morning and her hair began to come out. In other respects the medicine seemed to have no effect on her condition.

On the morning of March 23rd she took one of her old seizures. There was no convulsion but she became cyanosed and unconscious, and her bowels moved involuntarily. The pulse and respiration were rapid, but there was no rise of temperature. The cyanosis passed off after a minute or two, but the child lay quiet without taking notice of anything for two hours, when she had a second similar attack. After this she remained partially unconscious, restlessly tossing about until evening. Next morning she seemed quite well.

On April 6th she was measured again and was found not to have grown at all since January. The fontanelle was now scarcely perceptible. The child seemed well in every way.

[1] One grain of thyrocol is equivalent to five grains of dried thyroid gland.

On April 17th, between 9 and 10 A.M., the child, who had been quite well the night before, began to look dazed and was peevish and irritable. About half an hour afterwards she became unconscious and lay tossing about, not recognising anyone. The pulse was 174, very feeble, the respirations 40, and the temperature 99·2. There was no abnormality of the heart or lung sounds and no dyspnœa. This condition continued all day.

On the following day there was no change except that she vomited several times. The pulse was even weaker; the temperature did not rise. The pupils were equal and of medium size. There was no pain, only constant restlessness. Although quite unconscious, the child always struggled when touched and seemed to resent any attempt to examine her.

She remained about the same through the night, and died quietly on April 19th at 8 A.M.

Report of Post-mortem Examination

By Dr Jessie Macgregor

The body of the child was well-formed and well-nourished; and, with the exception of the dwarfed stature, there were no external pathological appearances.

The abdominal organs were normal, and there was no excess of fluid in the peritoneal cavity. The kidneys showed well-marked persistent fœtal lobulation.

On opening the thorax, the thymus gland was seen to be present and of large size. It occupied the space between the apices of the lungs, obscuring the trachea and in part also the thyroid gland, its lower border extended as far downwards as the level of the third rib, and its apices lay on the thyroid gland and great vessels. The appearance presented by the gland, when removed from the thorax, will be seen from the accompanying drawing (Plate IX., fig. 2). The thymus as a whole measured two and a half inches in its vertical extent, and three inches in its greatest transverse diameter. In its thickest part it measured three-quarters of an inch. It was somewhat irregularly divided into two unequal lobes, the right being somewhat the larger. In colour and consistency it was healthy, and there

were no adhesions or fibrous thickenings about its capsule. On microscopical examination of parts taken from various portions, its histological appearance was perfectly normal. The only abnormality in the structure, therefore, was its excessive size. The larynx was not flattened.

The thyroid gland was found in its usual position, and to the naked eye was perfectly healthy in all respects. Its right lobe measured $1\frac{3}{8} \times \frac{1}{2}''$, its left lobe $\frac{7}{8} \times \frac{3}{8}''$. On microscopical examination, a few of the acini were found rather more distended with colloid material than usual, but the epithelium was normal in all. It was only in a few of many sections taken from all parts of both lobes that such a condition was seen, and only in a few acini in these sections. The gland was thus almost perfectly normal, or perfectly normal, both macroscopically and microscopically.[1]

Beyond slight *post-mortem* collapse of the lungs there was no further pathological condition found within the thorax. The valves of the heart were healthy and well-formed. On opening into the cranial cavity, the skull was found to be normally developed. There was no excess of cerebro-spinal fluid, and the dura mater was non-adherent and healthy. There was deep congestion over the whole surface of the cerebrum, the veins being engorged and the small vessels markedly congested.

The convolutions were normal in form, number, and arrangement. They were not flattened, scanty, or atrophied. The membranes at the base of the brain were healthy, and, as far as could be observed macroscopically, the pituitary body was also normal. There was nothing pathological beyond the general deep congestion found in the brain, pons and medulla. The parts were not examined microscopically.

From this brief account of the appearances presented after death, it will be seen that the only abnormal conditions discoverable were, firstly, the great congestion of the brain, and secondly, the persistence of a greatly enlarged but otherwise healthy thymus gland.

[1] The parathyroids were submitted to Dr D. A. Welsh, who kindly examined them and reported: "The parathyroids are certainly somewhat unusually small, but otherwise I cannot find any special peculiarity either in the character or arrangement of their cells."

Remarks

From her physiognomy and general appearance this little girl seemed to belong to the somewhat rare class of well-proportioned dwarfs. Examples of this condition are from time to time exhibited to the public under such names as "General Tom Thumb," "Commodore Nut," "Queen Mab," etc.; and some have figured in history and in art.[1]

It is held by some that most, if not all, varieties of stunted growth depend in some way or other on defective action of the thyroid. This child was evidently not a cretin, although her sister was a typical example of that condition. She bore, however, in some respects a certain resemblance to a case of cretinism, and it was hoped that she might benefit by thyroid treatment to a greater degree than proved to be the case. The amount of growth in stature under prolonged treatment was not such as to encourage the idea that failure of thyroid secretion had been an important element in the causation of the symptoms. We were not therefore much surprised to find, at the *post-mortem* examination, that the thyroid was practically normal.

The nature of the seizures from which the child suffered and which in the end carried her off, is obscure. It is difficult to explain how the enlarged thymus could have caused them; but as several cases of enlarged thymus have been published where apparently similar attacks occurred during life, it seems quite probable that there was some connection between the two conditions.

APPENDICITIS[2]

By J. SCOTT RIDDELL, C.M., M.B., M.A.,
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IT occurred to me some time ago that a discussion at one of our meetings on appendicitis would be of the greatest value, as it would give us an opportunity of focussing on this serious, and unfortunately common disease, the clinical experience of

[1] See, for example, the portrait of Don Antonio L'Anglais, by Velasquez, reproduced in Charcot & Richer's *Les Difformes et les Malades dans l'Art*, p. 44.

[2] Read before the Medico-Chirurgical Society of Aberdeen.

members who have had exceptional opportunities of observing it in private and hospital practice, both from the medical and surgical points of view. That opinion was somewhat divided on the question of operation for appendicitis, I elicited at a meeting of the British Medical Branch some six years ago, when I brought forward an early case of relapsing appendicitis treated by the now approved method of removal of the appendix. Judging from the number of cases now sent for operation, and our added knowledge of the disease, I venture to hope that the views then expressed as to the value and necessity of operation in relapsing appendicitis may be found to be more in conformity with the ideas of some of my professional brethren now than seemed to be the case then.

It was originally intended that one or two physicians and surgeons should have given to-night introductory papers, but for some reason this has fallen through, and at the request of our secretary I agreed to put together a few notes of my experience in the treatment of this disease, in which I have always taken a great interest, notes which might serve as a text for the liberal and exhaustive discussion which I have no doubt will follow.

The pathology and operative treatment of appendicitis have been worked out in a little over a decade. It is surprising that the extreme frequency of the disease (for it is by far the most common of all forms of abdominal inflammation) coupled with the unsatisfactory old pathology of typhlitis, perityphlitis and paratyphlitis should not have drawn the attention of observers before. Typhlitis *per se* is a rare condition generally associated with impacted fæces, while perityphlitis also rarely occurs in cæcal ulceration. Clinical observation, coupled with what was seen on the operating table, established the fact that the appendix was in 98 per cent. of the cases the *fons et origo mali*, and led to clinical and laboratory work which have widened our sphere of usefulness and saved much human suffering and life. Did time permit, it might be interesting to follow the history of the development of our knowledge of this subject, but I have not had the leisure to compile such a statement, nor would you, I fancy, have the patience to listen to it if I had.

In this country the pioneer work, more particularly in con-

nection with the operative treatment of appendicitis in its relapsing form, was done by Mr Treves. It was meet that a surgeon should have been the one to attract the attention of the profession to this disease, for the diagnosis of it was first made on the operating table, and it has been on the operating table that the greatest triumphs in its treatment have been achieved. I propose to lay my modest experience before you from the surgical point of view, and I do not think I need apologise for so doing in view of what I have just said. The words of a great physician may be quoted to support me in this. He says:

"So impressed am I by the fact that we physicians lose lives by temporising with certain cases of appendicitis, that I prefer in my hospital work to have the suspected cases admitted directly to the surgical wards. The general practitioner does well to remember — whether his leaning be towards the conservative or radical methods of treatment— that the surgeon is often called too late, never too early." These are the words of Dr Osler, Professor of Medicine in Johns Hopkins University, and Physician-in-chief to the Hospital, Baltimore. He adds: "There is no medicinal treatment of appendicitis. There are remedies which will allay pain, but there are none capable in any way of controlling the course of the disease."

Gentlemen, there is no time to discuss the anatomy of the appendix, its variable points of origin, its length or its special inflammations such as the tuberculous variety and the perforative form seen in typhoid fever. Simple appendicitis will suffice for illustration and discussion.

Nothing strikes one who has operated on a few cases of appendicitis, more forcibly than the difference between that rudimentary structure in health and in disease. I show you an infant's appendix from a case of intussusception and, judging from what I have seen at laparotomies, in disengaging appendices from ovarian cysts and bowel, or in returning it from a hernia in which it was strangulated with the cæcum, I would say that the adult appendix is not much larger than the one I show you if you allow for the shrinkage it has undergone by hardening in spirit. Compare it now with this one removed from a case of relapsing catarrhal appendicitis, and

you will notice that the latter is much shorter and that its coats are immensely thickened.

The two great forms of appendicitis are the catarrhal and the ulcerative. In the former the lymphoid tissue is greatly increased, and the condition may either resolve or with the attendant plastic peritonitis which accompanies it may end in the formation of adhesions to the abdominal wall, to cæcum, to ileum, or to the bladder or pelvic structures. These adhesions have existed in all the cases I have operated on; they do not disappear, but are a prolific source of trouble and tend to promote relapses. The pain which they cause is great and is characterised by what is known as "appendicular colic." They may cause kinking and mechanical obstruction of the appendix, necessitating operation. Concretions or foreign bodies may be present in the catarrhal form, the former being much more common than the latter. They generally lie bathed in pus and have to be removed with great care. Granulation tissue may form in the interior of the tube which may get sealed up in whole or part by the process called, by Senn "appendicitis obliterans." I show you a specimen to illustrate this, where the lumen of the outer half was completely obliterated. If the end near the cæcum be obliterated, the secretion collects behind the obstruction and causes great pain. The other form is the *ulcerative*, where the destructive process extends through the mucosa or submucosa, and if it go no further, as in one case I had, may provoke only a plastic peritonitis such as I have tried to describe. If, however, it perforate, it produces appendicular abscess, which is fortunately localised as a rule by the delimiting adhesions formed in the early stage of the process. If the appendix has been adherent to the pelvic brim or wall posteriorly, it may perforate there and cause a large retroperitoneal abscess.

Acute fulminating appendicitis is the name given when the perforation occurs suddenly into the general peritoneal cavity, when a general septic peritonitis is set up, similar to but more virulent than in perforating gastric ulcer.

In the diagnosis of appendicitis there are some points worthy of notice. The tenderness over M'Burney's point is a valuable sign, but too much weight must not be attached to its presence or absence. From the variable situation of the

appendix one could hardly expect a constant point of maximal tenderness. Tenderness in the right iliac region might lead one to jump to a premature and erroneous diagnosis. If the "iliac phlegmon" be well marked mistake is hardly possible, but in relapsing cases or where induration round the seat of disease is not palpable, it is well to remember that other conditions occur which may simulate appendicitis. Thus it is never safe to diagnose appendicitis in a young female without a thorough vaginal examination or, what is better, a rectal examination, which is preferable for a thorough exploration of Douglas' pouch and the side of the pelvis. One is able then to assure oneself that the symptoms are not due to tubal, ovarian, or parametric inflammation. It must be remembered that the two often go together. Similarly hepatic and renal colic or inflammation of the gall bladder may lead one into error. I had a case recently of movable kidney with marked gastric symptoms; the case looked to be one of pyloric obstruction, for the kidney lay in the position where a pyloric tumour would lie. "Dietl's crises," so called after the physician who first described the marked abdominal symptoms often present in movable kidney, yielded entirely to nephropexy, and the lady is now in excellent health. I mention this because it is stated that "Dietl's crises" have been mistaken for appendicitis.

I have also had to remove the appendix for a patient whose case had, during one of the attacks, been diagnosed as obstruction from intussusception, or stricture at the ileo-colic junction; the case being one of relapsing appendicitis.

In the treatment of a case of simple appendicitis there are three questions about which there might be difference of opinion.

1. The first is the use of ice to allay inflammation and pain, as against fomentations and poultices. Heat, I think, is more grateful to the patient, and equally efficacious.

2. The second is the use of opium. I think that here, as in intestinal obstruction and strangulated hernia, it is often abused. It is apt, if given freely, to mask the symptoms and prevent the recognition of general peritonitis and of localised abscess formation.

3. Thirdly, Lawson Tait's plan of administering saline purgatives in cases of threatening peritonitis with great abdominal

distention following laparotomy, though of great service in such cases is, I think, utterly out of place in this condition where experience has shown that the colon is not loaded, and that peristalsis is more than likely to seriously aggravate the trouble. I used this method of treatment recently in a case of suppurative appendicitis, kindly sent me by Professor Finlay. It was called for by great abdominal distention and pain *a few days after the drainage of the abscess*, and had the most beneficial results.

The prognosis of simple appendicitis, so far as life is concerned, is very satisfactory. There are cases, however, in which the attack, though it yields to treatment, still leaves the patient with right iliac induration with attendant discomfort and pain. A second attack follows at a variable time from two to three weeks, up to a similar number of months, and in some cases the patient's life, from the distressful days he spends during the attack, from his discomfort and weakness during convalescence, and his constant dread of a fresh onset, becomes intolerable. In other cases the patient, after his first attack, may have only slight pain and right iliac discomfort, but complains of marked dyspepsia and constipation. The slightest indiscretion in diet in such cases is apt to precipitate an attack. In these patients, "symptoms" are too often treated and the cause left undisturbed. There are cases of relapsing appendicitis, and the attacks are not only a source of great discomfort to the patient, but a standing menace to him from the risk of perforation and abscess. Patients suffering from these recurrences should submit to operation during a quiescent period, and there is probably no operation in surgery that is more safe and satisfactory, if the grave issues at stake are considered, than this. The operative mortality is small, at the outside being under 5 per cent. in competent hands. In some hands it is not higher than 1 per cent. I have seen a fair number of recurrent cases, and I have never seen a bad result from operation. I have only operated on eight relapsing cases myself. In all of these cases the appendix was found diseased, and urgently demanding removal. In one case the appendix was greatly enlarged and distended, turned up on itself and adherent to the outer wall of the cæcum. In the others there were strong adhesions binding down the appendix to the parietes, ileum or pelvic wall, while

in one a fæcal concretion which I show you was found. In two cases appendicitis obliterans had partly sealed up the tube. All the patients made excellent recoveries, and profess themselves as absolutely cured. The method used in these cases was, after separation of the appendix from its mesentery and adhesions (this being best done with a blunt dissector, or small periosteum elevator), to turn back a cuff of peritoneum from the appendix close to the cæcal end, then to tie with catgut the exposed mucosa, remove the appendix and suture the cuff of peritoneum over the stump with fine silk. The method of simply ligaturing the appendix and cutting it off beyond the ligature leaves much to be desired, while the method of taking it off quite close to the cæcum, invaginating the stump into the cæcum, and suturing the cæcal wall over it, should, I think, be employed only in cases where the ulceration or obliteration extends close up to the cæcal wall.

A pertinent question arises as to how many relapses should be allowed. Statistics show that one case in three has a second attack, and that a second attack is more dangerous than an attack of enteric fever. Some advise removal after the first attack, or even during the first attack if there is not marked improvement by the third day. From what we know of the risks of recurrent attacks, of consequent perforation and general peritonitis or even localised abscess formation (where the mortality is considerable), most of us will agree with the dictum that only one relapse should be allowed to occur, particularly if that relapse follow closely on the first attack.

It has been advanced against the operation that hernia may occur in the wound. If the operation be conducted aseptically, and the layers of the parietes accurately sutured, first intention is the rule, and the risk of hernia is very small. If I do not weary you, I should like to mention one or two points about suppurative appendicitis.

1. *The help towards diagnosis got from a rectal examination.* This is not much thought of by some, but I have found it very useful in several cases.

2. *The necessity for such a case being early under the observation of the surgeon.* In any case of appendicitis that is not yielding to treatment, there is a great probability of abscess formation. This, however, may be difficult to make out. No

help is to be got from the temperature, for after suppuration has taken place, and specially in the most septic perforating cases, the temperature is often normal or subnormal. Much, however, may be learned from careful observation and examination of the patient, and as the surgeon is the one who is to take the responsibility of the case, he should have an opportunity of deciding as to the time of operation, and should, therefore, have the case under observation from the beginning of the unsatisfactory period. It is very unwise to wait until there is inflammatory œdema, reddening of the skin, and a distinct feeling of fluctuation. These are the cases that do badly, and are fruitful of general peritonitis and an increased mortality; while apart altogether from the question of mortality, there is the risk from delay of the abscess bursting into the bladder or rectum, with the formation of troublesome and even dangerous fistulæ. I have never seen an operation done too soon in such a case, but have seen the unsatisfactory results of delay.

But it is not sufficient to merely open the abscess. The abscess cavity should be explored, for it is generally found that there are secondary sacs which must be drained if a satisfactory result is to be obtained. In my last case I found a large cavity in the pelvis communicating through a small opening with the general abscess. Draining the outer abscess would have been of little avail. The pus in such cases is most offensive.

You are generally saved consideration of whether you will remove the appendix in suppurative appendicitis, for the very good reason that you can't find it in the mass of protective adhesions. Occasionally a gangrenous appendix is found hanging loose in the abscess cavity, when all that is required is to tie a ligature round its neck and snip it off. In one of my cases I removed from the wall of the abscess an ordinary pin encrusted with fæces.

If the patient be brought to you in too collapsed a condition to permit of the administration of an anæsthetic, the abdomen may readily be opened under cocaine or eucaine and chloride of ethyl. Two patients on whom I employed this method did not seem to suffer much pain. You cannot hope to cleanse the cavity by washing or even by scraping, as has been rather boldly advised. A single washing with drainage tubing in the

depth, and iodoform gauze plugging in the superficial sac, gives good results, though a mortality of about 20 per cent. must be looked for even in localised abscesses, the causes of death being usually general peritonitis, pylephlebitis, septicæmia, or secondary pyæmic abscesses.

In acute fulminating appendicitis, if a diagnosis has been made and the patient has been seen before the stage of absolute collapse, there is no doubt that it is the surgeon's duty to open the abdomen to find and remove the perforated appendix, and to wash out the abdominal cavity with sterile salt solution and to drain it.

No case of acute perforative appendicitis if left to nature recovers. With operation the patient has a chance almost similar to that after perforated gastric ulcer. One life saved by such interference is worth the trouble and anxiety of many very hopeless operations, and if life is not saved, there is still a feeling of satisfaction that all has been done that can be done, with the knowledge that the opening and flushing of the peritoneal cavity has done something to diminish the great discomfort from which the patient suffers, and to lighten the few distressful hours before his death.

CLINICAL EXPERIENCE WITH SALOL IN TREATMENT OF SMALL-POX [1]

By CHARLES BEGG, M.B., C.M., Bridge of Allan

THE following short statement of my experience in China with Salol is of interest at the present time, seeing that we are unfortunately dealing with an epidemic of small-pox in Hull and may at any moment be face to face with it elsewhere.

In speaking of Salol I pass over all reference to its use in intestinal cases, merely recording my strong conviction of its great value.

An observation I made during the progress of such a case led me to believe that it might be useful in quite another class of cases, and it is this I wish to call attention to to-night, not being aware of similar observations having been recorded.

My patient was taking four powders per day, each con-

[1] Read before the Medico-Chirurgical Society of Edinburgh.

taining 15 grains Salol. They produced a condition of slight mental depression, but the point which struck me was, that although he was, when in health, specially liable to severe irritation from mosquito bites, he now suffered no discomfort from their attacks and hardly noticed their attentions. This was not due to the mental condition. The puncture from the bite was plainly visible with a total absence of the characteristic reaction, neither swelling, heat, nor irritation being present.

I followed up this observation by giving the drug freely in cases of irritable skin diseases, acute eczema, prurigo, etc. of which we see many cases in the Tropics, and which on account of climate are far more trying to the patient and more urgently call for relief. In these cases I had my belief in the power of the drug confirmed. Then naturally I thought of small-pox, and in trying it obtained remarkable results. First, it kept the patient free from all sense of irritation without the slightest desire to scratch. Those who know anything of small-pox cases will at once see the great gain to the patient, preventing exhaustion from sleepless nights and drain to the system from weeping vesicles and pustules torn by scratching, avoiding danger to the eyes from soiled fingers, conserving energy for the most trying time of the disease, namely, the convalescence. But further, I found in the second place that almost all the vesicles terminated as vesicles, and the stage of maturation was absent except in a few isolated vesicles which matured in the usual way. In my last case, an Englishman, aged 60, with confluent small-pox, only two groups of vesicles, one on either upper arm, matured; they did so with perhaps greater energy than usual, taking on more the character of abscesses. In all the other vesicles the lymph dried up and there was an end of them. The relief to itching was most marked and could be demonstrated to depend on the drug, returning at once on reducing the dose.

I have not observed any evil effects from a daily dose of ʒi continued for long periods, and the mental depression quickly passes off with discontinuance of the drug, nor does it always appear in any marked degree.

Salol is accused of having caused death from 120 grs. given during eight hours. The death took place four days after. One would therefore like to know more particulars.

Salol is said to be of slow excretion, but allowed to be quickly decomposed into its component parts by the whole digestive tract except the stomach.

Both the phenol and salicylic acid are found abundantly in the urine, which becomes dark; but a large quantity must be given off by lungs and skin, because the characteristic sweet, fruity odour is very quickly perceptible in the room used by the patient, and is more pronounced than in the case of any other drug I know.

In acting as an intestinal disinfectant, it is acting, of course, quite outside the body; but the marked affinity it shows for elimination by lungs and skin would explain the above results of clinical observation by allowing it to act directly on the nerve terminals, and to reach the vesicles more surely than one could apply it with our hypodermic needle, a treatment long ago suggested in the case of abscesses by carbolic.

I have just found out that Salophen has been reported in the medical papers for 1897 as of use in pruritis, which would go to confirm my observations. *Salophen* was introduced to replace Salol on account of the ill effects thought to result from the elimination of phenol. It is said to quickly remove the pain and swelling in acute rheumatism, but does not prevent relapses.

I trust that those of you who may have the opportunity will not fail to bring your small-pox cases early and fully under the influence of this drug, in order that we may see if the results obtained in China are confirmed here. I know of no reason why it should not be so, for it is during the cool season in China the disease shows itself. Still there is always the charge to be met, namely, that one has perhaps been dealing with a milder type of the disease, or that the drug has been helped in its action by climate. Just as in my treatment of sprue, until I successfully treated cases from India, it was doubted whether the disease as seen in China was at best more than a mild form.

THE TREATMENT OF TAPEWORM WITH CHLOROFORM[1]

By J. MACKIE WHYTE, M.D.,
Physician to Dundee Royal Infirmary

THERE is no ideal anthelmintic. The extract of malefern which has for long been the prime favourite is often ineffectual, and occasionally produces very untoward symptoms. After some experiences of such a sort, I was induced by the strong advocacy of chloroform by various physicians, chiefly American, to give it a trial. Theoretically, nothing could appear simpler. The chloroform acted directly on the parasite, either killing it or, at least, anæsthetising it, so that a laxative given soon after caused its expulsion.

My first case was a little girl of twelve. A dose of twenty minims, made up with half an ounce of glycerine, preceded and followed by evacuants, was perfectly successful. There was no disagreeable after-effect, and the patient did not go to sleep. For a time there was a feeling of heat in the stomach. The head was not found, but there has been no reappearance of segments.

The second case was that of a man, R. S., 37 years of age, transferred to my ward in the Royal Infirmary from Mr Greig's, where he had been under treatment for prostatic abscess. He had passed bits of tapeworm for a twelve-month, and had been ineffectually treated with malefern. Five minutes after taking 3i of chloroform he went to sleep, and remained asleep for an hour. He then got a dose of castor oil, and passed a great length of tapeworm, but in three months segments again showed. I tried chloroform again in more cautious doses, first 40 ℥, a few days later 50 ℥, but without further effect than a pleasant sleep. One and a half drachms of ext. filicis liq. in Duncan & Flockhart's capsules were then given, and proved effectual. This was two and a half years ago. No recurrence since.

A son of this man's, a boy of nine, was recommended to me by Mr Greig for treatment. He had been troubled for four years, and had got malefern without cure. Three

[1] Read at a meeting of the Forfarshire Medical Association.

successive doses of 20 ♏ chloroform had no effect whatever. As I believed the dispensing was at fault, I made him get his next dose, 30 ♏, from another druggist. The boy went to sleep in three minutes, and slept for an hour and a half. After a dose of syrup of senna he expelled most of the worm. The father said he found the head, but he was mistaken, as the segments subsequently began to appear. Cure was finally obtained by malefern.

The fourth case was a woman aged twenty-seven, who was treated in the Infirmary. After her second dose of ʒi of chloroform, three days subsequent to the first, a length of 17 feet was passed. The head was not found. No disagreeable effects, except a feeling of heat and of sickness. The case cannot be called a cure, as it has not been possible to trace the patient.

The fifth case was in private, Mrs S., recommended to me by Mr Greig. After preparation the night before she took 40 ♏ chloroform at 7.45 A.M., and a dose of castor oil at 8. She fell asleep at 11 after vomiting, and slept for an hour. Then there was purging, and most of the parasite was passed. In two months, however, segments appeared, and after another ineffectual attempt with chloroform (which caused a deep sleep of an hour and a half), liquid extract of malefern was given. The result was apparently successful, but the present condition is unknown.

The last case was that of a man, J. W., treated in the Infirmary. Chloroform was given twice, and caused a sleep of half to three-quarters of an hour. No effect on the parasite. ʒiss ext. fil. liq. (D. F. & Co.) was successful. No recurrence during the past two years.

In this list then, there is only one success, the first, to be put down to chloroform. One other is doubtful.

Perhaps it would have been better to give the purgative along with the chloroform, either castor oil, or as Graeser recommends, a minim of croton oil.

In the new journal, *Die Therapie der Gegenwart* for September 1899, Prof. Leichtenstern gives his experiences in the use of chloroform as an anthelmintic. The parasite was *Tænia mediocanellata* (*saginata*). Out of thirteen cases, only one proved successful; the others were almost all treated

subsequently with success by the administration of ext. filicis liq. in doses of ʒii to ʒiiss. The chloroform was given as recommended by Graeser and Thomson to the extent of ʒi, usually in two or three portions at intervals of one or two hours. In most of the cases there were no disagreeable effects apart from occasional headache, giddiness, or sickness, such as might be expected with any anthelmintic. In others a very deep sleep occurred, lasting in one case for twenty-four hours, and in two cases such severe collapse occurred that there was considerable anxiety for the patient's life. The conclusion is that chloroform is of no use in the treatment of Tænia saginata. As for Tænia solium, while he will not deny it may be as efficacious as some allege, he thinks less dangerous remedies such as kousso should be employed.

I am compelled then to agree with this conclusion as to the inefficacy of chloroform. And although I did not meet with any alarming effects, still one does not care to risk the onset of a state of collapse, especially in private practice where the patient is not under trained and watchful supervision. It is interesting, nevertheless, to note the usual effect of the administration of chloroform by the mouth. A dose of a drachm produced rapidly in most cases a comfortable deep sleep of about an hour's duration. As a matter of fact, chloroform has been given with this intention. Dr Davidson Scott of Washington, for example, says he gives it in cases of "congestive chills," etc., in doses of a drachm upwards, repeating it every two or three hours. And it is said to abort a malarial attack if given in a dose of a half to one drachm before the expected time for the occurrence of the chill (Wood's Therapeutics). Death from administration of chloroform by the stomach has usually resulted from very large doses (a half to two ounces), but Hirsch is quoted, in Leichtenstern's paper, as citing a case of death in a four-year-old child from forty-five minims. I have also found difficulties on the part of our good friends the druggists; one careful man seemed to take so long to mix up the glycerine with the chloroform that most of the risk was dissipated into the air of his shop. And it is not pleasant to be rung up by telephone from a shop, your patient being perhaps within hearing distance, and to be asked whether you really mean such and such a dose, for the B.P. says so and so.

Occasionally alarming symptoms occur with the giving of malefern. They are best avoided, I think, and the best results are got by using the preparation of a good maker. I generally use Duncan & Flockhart's capsules to the extent of ʒiss, but ʒii or ʒiiss are safe doses.

NOTES ON A CASE OF LYMPHADENOMA

By JAMES BURNET, M.A., M.B., Ch.B.,
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THE treatment of lymphadenoma is, as a rule, so hopeless, that I may be excused for bringing before the notice of the profession a case which came under my care some time ago. My success in this particular instance was most encouraging, and I trust that the case may be found sufficiently instructive to warrant its being placed on record.

The patient, as is usual in these cases, was a male, æt. 51. He was a hawker, but formerly had been a farmer in the south of Ireland, of which country he was a native. He was married and had two children, both of whom were alive and healthy. His food had always been good, but he had often taken alcohol to excess. His home surroundings were very satisfactory, but his occupation exposed him to all kinds of weather. Since childhood he had had no serious illness until three years ago, when he had a severe attack of influenza. Since then his health had been completely broken down.

When I first saw him he complained of weakness and great sweating which had been getting progressively worse for a year past. About this time he also accidentally discovered that the glands in both axillæ were enlarged. He had consulted several medical men, but they all seemed to look upon his condition as one of asthenia consequent on his influenzal attack. He was ordered tonics of various kinds, but none of them seemed to have the slightest effect upon him.

When I saw him he certainly presented a very striking appearance. His face had a cachectic aspect, and his eyes were sunken, with dark areolæ under the lids. He looked very tired, worn, and weak. His trousers were fastened at

the top with a piece of string to the buttons. The glands in the axillæ and groins were enlarged and somewhat matted together. Under the skin covering the thorax a few small glands were palpable. The glands in the neck were quite unaffected. The abdomen presented some interesting features. There was absolute dulness, except in the epigastric region, where a tympanitic note was obtained. The bowel was evidently pushed up towards the xiphisternum, and its outline could be seen without difficulty. Moreover the abdomen was hard and tense like a board. The lower part of the sternum was greatly thinned, and felt like a piece of cartilage. In the left hypochondrium the ribs were pushed out as if by some tumour mass beneath. The temperature was quite normal, as were also the urine and fæces. The pulse was quite regular, though somewhat feeble. One thing which struck me was the enormous amount of sweat excretion. The clothes looked as if they had been taken out of a tub of water.

I commenced treatment by putting him on simple but nutritious diet, and keeping him in bed for a certain part of each day. Meantime I made a careful examination of the blood. I found the red corpuscles numbered 3,800,000; the leucocytes were, if anything, increased in number, the multinuclear elements being distinctly marked in some of the films, and in a few cases nucleated red cells were obtained. The hæmoglobin was reduced to 35 per cent. On the whole, therefore, it will be seen that the blood was more of the character displayed by a case of chlorosis than any other condition.

Having satisfied myself as to the real nature of the case, I at once put the patient on iron and arsenic pills. These I continued for ten days without any result whatever, the sweating and extreme weakness still being marked, and the condition of the blood unchanged. I now tried the effect of mercurial inunctions over the axillary regions and the abdomen, and told him to continue taking the pills. At the end of a week the glands in the axillæ seemed somewhat less hard, but the abdomen was still dull and very tense. I resolved now to stop the pills and put the patient on five minim doses of the Liquor Arsenicalis three times a day, along with port wine, and at the same time to continue the inunctions. A week later the patient had lost seven pounds in weight, the abdomen was

much softer, and a tympanitic note was obtained nearly to the umbilicus.

Encouraged by this result of treatment, I resolved to push the arsenic and mercurial inunctions. A few days, however, showed me I had been too rash, for the patient's temperature rose slightly, and he now began to complain of tender gums and some pain on swallowing. I at once stopped the inunctions, but ordered ten minims of the Liquor Arsenicalis thrice daily. The condition of the mouth quickly improved, and I resumed the inunctions, but with greater care on this occasion. About a week later I found he had lost a stone in weight since he came under my care, and his abdomen was quite tympanitic save over a small area in the umbilical region. His sweating —a marked symptom in the case—had ceased, he was taking his food better, and the swelling in the left hypochondrium was greatly diminished, as were also the enlargements in the axillæ and groins. The abdomen had now assumed its normal girth, for the patient could now button his trousers at the top. He also told me that he breathed with greater freedom, and he said that he had not felt so well for months. He had now been six weeks under my care, and the red blood corpuscles had risen to 4,750,000, and the hæmoglobin to 65 per cent. He could go about and enjoy life just as he used to, and he felt quite a new creature. I now advised a change to the country, and the patient thought he could not do better than go back for a time to his native place in Ireland. He did so, and was ordered to take five minims of the Liquor Arsenicalis thrice daily for a few weeks, and to use the mercurial ointment every night. Two months later his son called on me, and told me that his father was doing well, and was showing no recurrence of his symptoms, nor any evidence of further glandular swelling.

That this patient recovered under therapeutic remedies is absolutely certain, but I attribute no small part of his improved condition to the fact that I enjoined him to sit out for an hour or two every forenoon in the sun. I strongly believe in the effect of direct sunlight on all patients affected by diseases of the blood, and have seen marked improvement accrue from it in cases of ordinary anæmia. Of course I do not pretend to say that this patient is cured completely—far from it, for he has still lymphadenomatous masses in the axillæ, groins, and

mesentery; still his condition is so much improved that I feel satisfied with the results of the treatment employed.

It will be noticed that the iron and arsenic pills at first prescribed did not seem to suit at all, but that the Liquor Arsenicalis given alone had a marvellous and almost immediate effect. One lesson I learnt was that mercurial inunctions must be used with very great caution lest stomatitis and subsequent enlargement of the glands of the neck may result. It is well, therefore, to stop the inunctions on the first evidence of mouth trouble becoming displayed.

It will be interesting to know if the above line of treatment proves equally efficacious in other cases of lymphadenoma.

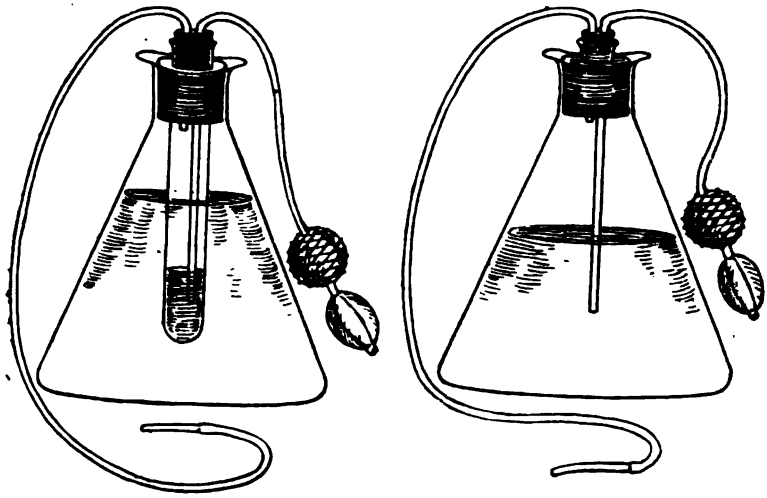
NOTE ON A HOT-AIR NEBULIZER FOR VOLATILE MEDICAMENTS

Communicated to the Medico-Chirurgical Society, Edinburgh

By A. DOUGLAS WEBSTER, M.D., F.R.C.P.Ed., Edinburgh

THE apparatus as figured consists of an Erlanmeyers flask of capacity about 500 c.c., into the neck of which is fitted a cork with a hole in the centre. Through this is inserted a test-tube closed by a rubber cork having two holes in it. Into these holes are fitted two finely-pointed glass tubes, one of which passes nearly to the bottom of the test-tube, while the other only just passes through. An ordinary rubber bellows is attached to the longer piece of glass tubing, and to the other is fastened about a foot and a half of rubber tube. The test-tube being removed, the flask is three-quarters filled with water of the desired temperature, or the water may be heated by a spirit-lamp. The volatile medicament, or one dissolved in any fixed oil, is then poured into the test-tube, filling it about a quarter full, so that the long glass tube passes below the surface of the fluid. (Solutions in spirit or water are naturally not suitable for a dry hot-air spray.) The test-tube is then replaced and the medicament is soon warmed to the temperature of the water, which is usually from 120 to 180° F., the effect being that when the bellows are worked a very finely divided and warm spray of the pure remedy is driven out in

the form of a cloud. Many different remedies can be used in this apparatus. Among these is one composed of a mixture of turpentine two parts, eucalyptus oil one part, and a 10 per cent. solution of menthol in paroleine one part. It has been found useful in several cases of chronic eustachian deafness in adults, and in children with enlarged tonsils and adenoids with deafness; in both cases the catarrh was relieved and removed. Pure cresolene, cresol, and other coal-tar products thus vaporised have been found effective in whooping-cough and diphtheria. Menthol in paroleine or in olive, almond, or vaseline oil, 5 to



15 per cent. (the latter is liquefied by the heat of the water), as mediums, were specially satisfactory. These as well as pure terebene, pumilene, and pinol have been found applicable in catarrhs. In ozœna, septic sore throat, and phthisis, sprays of pure carbolic acid or creasote have been tried with success. Turpentine, cajuput, pine, eucalyptus, gaultheria, and other aromatic oils were exhibited.

The spray is of simple construction, and by a slight adaptation of the cork can also be used as an ordinary vapour spray and inhaler; in this way it is particularly efficacious, as the volume of the vapour is under control, and can be directed to the affected area of mucous membrane. This adaptation is also suitable for the application of remedies dissolved in water

or spirit. Ipecacuan wine in croup and compound tincture of benzoin in tonsillitis have been useful, one teaspoonful of either in the flask, one half filled with hot water at the temperature of about 140° F.

NOTE OF A LARGE SOLID INTRA-LIGAMENTARY TUMOUR REMOVED BY ABDOMINAL SECTION [1]

By N. T. BREWIS, M.B., F.R.C.S.Ed.,
Lecturer on Gynecology, Royal Colleges School of Medicine, Edinburgh

SOLID neoplasms growing between the layers of the broad ligament may develop from the uterus, the connective tissue of the mesometrium, the Fallopian tube, the round and ovarian ligaments. The new growths met with are fibromata, myomata, fibro-myomata and sarcomata. Tuberculosis and cancer are always secondary. Sarcomata develop from the connective tissue normally present. Berry Hart and Bland Sutton each report a case. I have met with two cases growing in the cellular tissue of the recto-vaginal septum. Bland Sutton in his valuable book "Surgical Diseases of the Ovaries and Fallopian Tubes, including Tubal Pregnancy," at page 350, says myomata have not been observed before the thirty-fifth year, but Doran disproves this statement in his paper entitled "Cases of Fibroma and Fibro-myoma of the Broad Ligament," published in Vol. XLI. of the Transactions of the Obstetrical Society of London. After attaining a certain size myomata may undergo cystic or sarcomatous degeneration, and grow with great rapidity. Sarcomata in the broad ligament as elsewhere may occur at an early age.

Intra-ligamentary solid growths are so rare that I thought the society would be interested in the following case:—

M. H., aged forty-nine, an unmarried lady, was sent to me by Dr M'Callum of Wooler. She complained of an abdominal swelling which she first noticed in June 1897 as a hard swelling about the size of an orange presenting in the right iliac region. The swelling increased gradually and slowly till three months ago, from which time it has grown rapidly. The patient has always enjoyed good health, and the tumour has

[1] Read before the Edinburgh Obstetrical Society.

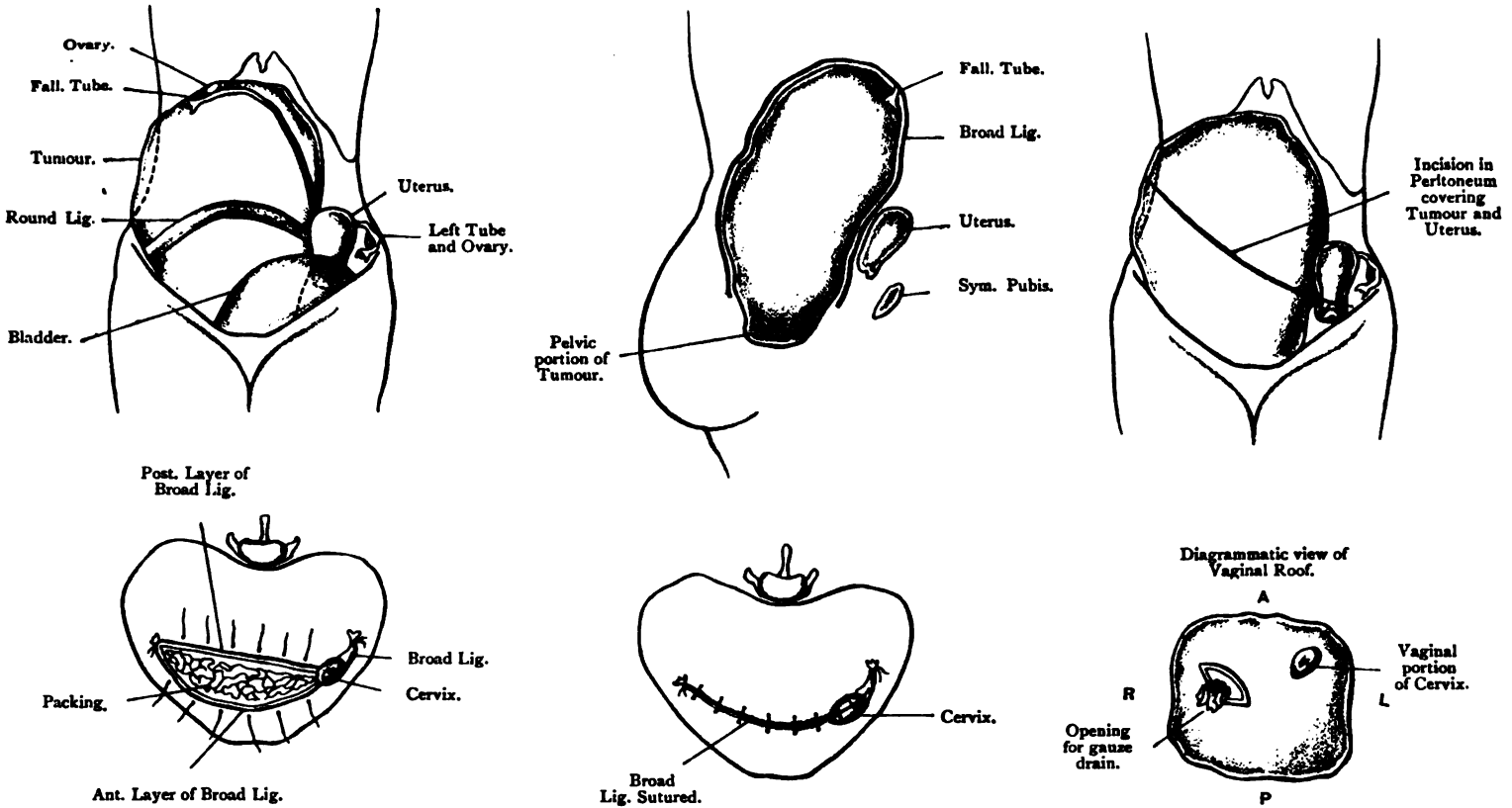
never caused any pain in the abdomen or pelvis, but occasionally a sharp pain has been felt in the back, coming suddenly and lasting for days. Menstruation has recurred regularly every three weeks, lasting two or three days, the amount, which never was abundant, has been scanty during the last few years. She has always menstruated with comfort. There has never been any intermenstrual discharge.

About every three months during the last three years the patient would for three and often four days pass little urine—less than a teacupful in twenty-four hours—this caused her no discomfort. The patient has always been subject to severe bilious attacks, but these have been less frequent of late years. The bowels have always been constipated, but not more so lately. She has not lost flesh.

Physical Examination.—The abdomen is distended to the size of a seven months' pregnancy by a tumour of firm consistence. The swelling is ovoid in shape, and presents a regularly smooth surface, except in the left iliac fossa where a hard lobe about the size of an orange is felt. The tumour is not moveable. No fluctuation can be made out and no bruit is heard. Per vaginam the tumour is found to extend low down into the right side of the pelvis, and in this position feels hard and nodular. The uterus is made out to be in the abdomen, pushed upwards and to the left by the tumour, and occupies the position of the lobe before mentioned. The external os uteri is at the level of the pelvic brim. The sound passed into the bladder goes upwards and to the left, and its point is felt about three inches above Poupart's ligament.

As the tumour was growing rapidly there was no hesitation as to treatment.

Operation.—On 25th October 1899, the second day of the menstrual period, the abdomen was opened by a long incision, the tumour turned out and its relations recognised. It was found to be a solid tumour growing between the layers of the right broad ligament. The Fallopian tube stretched over its summit; the right ovary compressed flat lay behind the outer end of the tube; the right round ligament, greatly hypertrophied, coursed across the anterior surface, while the bladder covered the lower part of the tumour and uterus. The latter, somewhat enlarged and containing a small fibroid embedded in



FIGURES ILLUSTRATING THE POSITION OF, AND THE OPERATION FOR, REMOVAL OF INTRA-LIGAMENTARY TUMOUR IN DR BREWIS' CASE.

its anterior wall, lay in the left iliac region. The left ovary and tube were healthy. The tumour descended deep down into the pelvis. The left ovarian and round ligament vessels were first tied and the broad ligament divided. An incision was then carried through the peritoneum over the anterior surface of the uterus and tumour. The broad ligament was now peeled off the tumour and the latter shelled out of its bed after the right ovarian artery had been tied. The uterine arteries were then secured and the body of the uterus removed. There was much oozing in the cavity left by the removal of the tumour. Iodoform gauze was packed into the cavity and brought through an opening made in the vaginal roof. The anterior and posterior layers of the broad ligament were then stitched together over the gauze and stump of the cervix. The abdominal wound was closed without a drainage tube. The patient stood the operation well and was put to bed with a pulse of 72 per minute. Her progress since has been satisfactory. The temperature remained normal till the fifth day, when it rose to 100, but came down to 98·4 after the packing had been removed.

Description of the tumour.—The tumour at the time of removal was pale red in colour and weighed 18lbs. Its shape is oval, laterally compressed; it measures 13 inches by 10, and its circumference is 27 inches. It is chiefly made up of one large lobe, which is divided at its base by two deep sulci into three smaller lobes. The tumour is covered by a capsule, which is thin over the general surface, but is in places gathered into thick bands which compress the substance of the tumour in the form of furrows. These furrows are well marked on the summit and on the anterior and posterior surfaces. Anteriorly the centre is compressed and branches are sent out laterally. The greater portion of the surface is smooth and even in contour, the summit is more irregular and presents two small bosses. The consistence is firm as a whole, in some portions it feels somewhat hard and nodular, in others doughy. The tumour had no pedicle.

On section the cut surface presents a greyish-white appearance, studded over with numerous small bleeding points. The surface of the outer third of the section is even, while that of the inner two-thirds shows numerous pits due to mucoid softening. At one part there are two irregularly shaped cavities

filled with myxomatous looking material. Since removal much blood and watery fluid have oozed out of the tumour.

Microscopical examination of a portion of the tumour by Dr W. B. Drummond :—

"A section removed from the outer portion of the tumour presented the ordinary appearance of a fibro-myoma in which the fibrous tissue was relatively abundant. The bundles of non-striped muscle presented the usual wavy character and crossed each other in all directions. The entire section was extremely vascular, and connective tissue cells were very abundant round about the blood-vessels, and in parts of the section were collected into small groups. Here and there a tendency to myxomatous degeneration could be made out, but this process was apparently more marked in the central portions of the tumour; from which, however, sections were not made."

Remarks.—The following points in this case appear to me worthy of notice :—

1. The absence of Symptoms.—The patient carried this heavy solid tumour without any inconvenience, her general health was in no way affected, and menstruation occasioned no discomfort. The occasional marked diminution of urine, due probably to pressure on the ureters, was the only symptom which could be attributed to the growth.

2. Diagnosis.—The position of the tumour and the displacement of the uterus and bladder pointed to the growth being situated between the layers of the right broad ligament. Its consistence excluded ovarian or broad ligament cystomata. The position and consistence of the growth indicated either a solid tumour of the mesometrium or a fibroid growing from the side of the uterus.

The differential diagnosis between these conditions must always be difficult. Both may cause the same uterine displacement. The length of the uterine cavity is not a certain guide, as the uterus may be greatly enlarged by a fibroid without any corresponding increase in the dimensions of its cavity. In the case I record no vascular bruit was heard, but this sign is often absent in uterine fibroids. The diagnosis can only be settled after opening the abdomen, even then the origin of the tumour may be doubtful.

3. Operation.—The operation was quite a straightforward

one. Injury to the ureter was the chief risk. It was feared the ureter might be placed above the tumour, and great care was exercised in peeling off the layers of the broad ligament. After the tumour had been removed we were pleased to recognise the duct uninjured. There was much oozing from the vessels of the mesometrium. This was effectively controlled by ligation of the ovarian and uterine arteries on both sides, and by packing gauze tightly between the layers. Bringing the gauze into the vagina instead of through the abdominal wound gave better drainage and enabled us to stitch the anterior and posterior layers of the broad ligament over the gauze so that the drain was entirely extra-peritoneal.

Meetings of Societies

Aberdeen Medico-Chirurgical Society

Discussion on Appendicitis

A MEETING of this Society was held on January 11th, Dr W. H. Williamson, President, in the chair.

The evening was devoted to a discussion on appendicitis, opened by Dr J. Scott Riddell, whose paper appears in full in another part of this Journal.

Professor Finlay preferred the old names *typhlitis* and *perityphlitis* to *appendicitis* as being more comprehensive: he referred to a case in which, while the appendix was intact, there was a stercoral ulcer of the cæcum, with perforation at its tip. He thought cases might be clinically divided into three principal classes: (1) cases, often in adults, with moderately acute symptoms and distinct swelling and tenderness, which begin to improve in a few days and are suitable for medical treatment; (2) cases with more acute symptoms, often in adolescents, especially boys, due to sharp local peritonitis, generally from perforation—these should be operated on; (3) fulminating cases—in a lad, say of 14 or 15, perhaps after some athletic exercise, starting with severe pain in the right iliac region, collapse and signs of rapidly spreading peritonitis—these are hopeless unless operated on at once. With regard to diagnosis, sudden abdominal pain in the right iliac region, followed by peritonitis, in a person of school age, is probably perityphlitis if injury and tubercle can be excluded. As regards surgical interference, it is much safer to operate unnecessarily

than to leave doubtful cases unoperated upon; most cases, however, belonging to the first class do well without operation. All cases, therefore, should not drift over to the surgeon, but a surgeon should be associated with the physician in watching the course of the case. In relapsing cases, it is better not to allow more than one relapse. Indications as to the time for operative interference are to be looked for in the abdominal condition, the general condition, *e.g.* a rapid, feeble running pulse, etc.; pain is not of so much value. The appendix itself, unless gangrenous, may be left in many cases, as it may shrivel up and give no more trouble. Dr Finlay referred to one case, in which an operation failed to disclose anything, but the autopsy showed the appendix, with perforation at its tip, lying in an abscess-cavity at the back of the cæcum; he thought it would be well in such cases, when found, to make drainage through the loin.

Dr T. Milne drew attention to the enormous increase in the number of cases of appendicitis within recent years; there is practically no allusion to it in text-books of thirty years ago. He suggested that the over-exertion of cycling might be an ætiological factor. All his cases except one—which was probably an example of the fulminating class and ended fatally—had recovered without operation.

Dr John Gordon had got recovery in the great bulk of his cases without surgical interference. Of three cases operated on, two died and one—a relapsing case—made complete recovery. In one of the fatal cases, the condition disclosed at the operation would make him doubtful as to the advisability of surgical interference in a similar case. He referred to the opinion which had been expressed, that this disease was more common in the south than in Aberdeen, and that the hurried bolting of food of the city business man might be a causal factor.

Dr A. Westland agreed with Dr Finlay that the name *appendicitis* was used too widely: he was strongly convinced that many such cases were cases of inflammation of the cæcum. In favour of this were these points:—(1) the large size and the shape of the abdominal tumour; (2) towards the end of the disease, the passage of large masses of mucus, sometimes like casts of the cæcum. As other parts of the large bowel might be inflamed, *e.g.* the sigmoid flexure and the region of the left hypochondrium, inflammation of the cæcum was *a priori* not improbable. All his cases, except those of the fulminating type, had recovered without operation. Small doses of opium were useful in relieving a great deal of the discomfort and pain—made the pain "appear further off"—without obscuring the features of the case.

Dr Rose deprecated the tendency to unnecessary surgical interference: all his cases had recovered without operation. Many cases of so-called "relapse" were due to insufficient time being allowed for recovery: relapsing cases should be treated medically for a long time—one relapse did not call for operation. He advocated the free use of opium to relieve the pain and paralyse the bowel.

Dr Edmond, from his experience, could not understand how men who had been practising for a long time had not seen cases with pus-formation requiring operation. He thought the hurried bolting of food was an important ætiological factor; he agreed with Treves' view of the pathology. Most cases belonged to Dr Finlay's first class, and recovered in ten or fourteen days without operation; still, it was wise to associate a surgeon with the physician in every case. The signs of probable pus-formation (and therefore indicating operation) were the appearance of the patient, increase in the local abdominal signs (*e.g.* more swelling and dulness and diminished movement), and the temperature remaining high or rising again after it has fallen. One relapse did not necessarily indicate operation, but this should be advised: (1) if there had been several relapses, which tended to come nearer together, and the last relapse a bad one; (2) if a distinct local tumour could be felt between the attacks. He thought that, as the cause was so often a bolus, an enema or aperient (*e.g.* castor oil) was often good at the onset of an attack; later, sufficient opium to relieve the pain.

Professor Stephenson thought the character of the abdominal tumour was not sufficiently considered, and that it was probably an important point of distinction between appendicitis and other inflammatory processes in that region. All his cases had been single attacks, and required no operation. Relapses were often due to imperfect recovery; freedom from pain and tenderness was not sufficient to show convalescence, there must also be freedom from matting of the bowel, and, above all, the temperature must not only fall, but *remain* steady.

Dr Marnoch made remarks on the technique of the operation. In recent acute abscesses the cavity should not be washed out, in case the adhesions might be broken down and a local peritonitis thereby become general. For the same reason, if the appendix were much matted, it should be left alone; but if the appendix could be removed with ease, this should be done in all cases, because the lumen at the extreme tip might remain unobliterated, and form a focus giving rise to a relapse. He referred to one suppurative case in which the suppuration had gone on for a long time after free opening.

Dr Fortescue advocated early operation in relapsing cases,

as there were fewer adhesions and the operation was therefore easier than later. He referred to one case with two ulcers of the cæcum and the appendix intact, successfully operated on.

The President thought that the hurried bolting of food was a more probable ætiological factor than deficiency of teeth, as the disease seemed to be more frequent in America, where more attention was given to the teeth.

The Edinburgh Medico-Chirurgical Society

THE fifth meeting of the session was held on Wednesday, February 7th, Dr Dunsmure, Vice-President, in the chair.

Mr Cathcart showed a boy who had suffered from chronic intestinal obstruction with alternating diarrhœa and constipation. The cæcum was excised and the bowel was found to be contracted to the size of a No. 6 catheter. The lesion was believed to be tubercular.

Dr Bruce showed a case of insular sclerosis in which there was marked exaggeration of the deep reflexes, especially a marked ankle and adductor clonus which sometimes alternated with each other on patient pressing his toes on the ground.

Dr Norman Walker showed a girl of twenty years of age with a chronic ulcer on the forearm which almost healed and again broke down. It presented resemblances both to tubercle and syphilis, but there was a history pointing to neither. Anti-syphilitic treatment had produced slight improvement.

Mr Caird showed two small sequestra the size of hazel-nuts removed from the alveolar border and hard palate of the upper jaw. They were both obtained from patients who had undergone a mercurial course during the primary and secondary stages of the disease. The first, L. W., an abstainer, infected eleven years ago in India, was treated for nine months by pill. Seven years ago destructive throat symptoms appeared—loss of soft and greater part of hard palate and of upper lip, with extensive pharyngeal ulceration and separation of sequestrum exhibited.

The second, M. L. W., infected eight years ago, was treated by the mouth and by inunction on the continent. There were no secondary symptoms till later, and then only in the mouth. Mr Caird also showed a specimen of extensive syphilitic necrosis involving the greater part of the frontal bone and both parietal bones. The patient, aged twenty, suffered from hereditary syphilis and showed characteristic manifestations of the disease. For many years she had sinuses discharging fœtid pus from the sagittal, coronal, lambdoid and temporal sutures. The preparation was removed after death and dis-

closed a huge cavity in which the surface of the brain covered with its thickened dura mater lay at the upper level of the squamous portion of the temporal bone. Various futile attempts had been made during life to perforate or remove the sequestra.

Mr David Wallace exhibited a radius showing a fracture of the lower end with unusual displacement resembling Smith's fracture.

Mr Cotterill showed a simplified form of trephine, consisting of two pieces only, which could be readily sterilized and was not liable to get out of order.

Mr Cathcart showed a method by which sterilized gauze might be handled without touching more than was required for immediate use. The gauze was rolled in balls unwinding from the inside as in his method for catgut.

Mr Caird read a "Short note on a case of ligature for femoral aneurism (with patient)," which will appear in a future number of *The Scottish Medical and Surgical Journal.*

Mr Wallace read a "Short note on hæmaturia due to bilhartzia hæmatobia," which will also appear in a future number.

Dr Allan Jamieson recalled a case of bilhartzia in a male patient who suffered from no symptom but hæmaturia.

Dr Underhill had observed bilhartzia in a female patient, who with every attack of hæmaturia had severe pain. Boracic acid gave some relief, and caused no symptom although continued some time.

Dr Gulland believed that the priapism in Mr Wallace's case might be due to the presence of the parasite in the seminal vesicles.

Dr W. W. Ireland read a paper "On the increase of nervous diseases and insanity," which will appear in this Journal.

Dr Clouston stated his belief that the increase was only apparent, nervous diseases being more accurately studied, and because many slight cases of insanity were admitted to asylums which formerly were not considered insane.

Edinburgh Obstetrical Society

THE fourth ordinary meeting was held on February 14th, Dr J. W. Ballantyne in the chair. Among the specimens were a hydatid mole, the head of a child delivered by basilysis, and two uteri with submucous fibroids removed by pan-hysterectomy—all shown by Professor Simpson.

Dr J. W. Ballantyne showed a fœtus with absence of radii

and deformity of thumbs, an anencephalic fœtus, and photographs of some historical teratological phenomena. A myxomatous ovum was shown by Dr Keppie Paterson.

There are no papers to which the Society listens with greater pleasure than those by Dr Macvie, and his paper on "A case of eclampsia having a peculiar puerperal history, and observations on coma," was no exception to the rule. The patient was thirty-five years of age, and of markedly neurotic temperament. The paper specially related to her fourth confinement; the other three labours had been easy, but the puerperia complicated by the recurrence of a stuperose condition at the end of the first week. In the second confinement this was so aggravated that signs of returning consciousness only began to appear when death seemed imminent. Dr Macvie decided to diminish the risk of these attacks by pushing chloroform in her fourth labour, both to prevent pain and to cut off the remembrance of it. On being called he found the os half dilated, the membranes still unruptured, liquor amnii in excess, and a face presentation—right-mento-posterior. The ankles were œdematous and other indications pointed to a risk of eclampsia, but in spite of this he decided to go on with the chloroform. He attempted to convert the presentation to a vertex, but had to be content with a right-mento-anterior. The passages being dilatable and roomy, delivery was soon effected by forceps. At 10 A.M. she had wakened up and taken some nourishment. At 4 P.M. she complained of a dizziness in the head; this was quickly followed by eight or ten convulsions. At 6 P.M. he found her unconscious, with flushed face, laboured and stertorous breathing, the pupils dilated, and the pulse small, hard, and rapid; she had shown no signs of consciousness since the first fit. Sedative treatment was evidently valueless here. The uterus was douched, and the rectum washed out and filled with normal saline solution. Four ounces of urine were drawn off and found loaded with albumen.

With reference to the best line of treatment, Dr Macvie then discussed the causes of the onset and cessation of a fit. To "attend to the emunctories" is an excellent maxim, but one wishes to have a more definite objective. The symptomatic sequence of a convulsion implies a causative sequence. Can one attack the proximate cause of the last straw—the coma? To reach it is to "relieve Ladysmith." It will doubtless be said that a rapidly increasing cerebral anæmia will cause the whole sequence of a convulsion. Dr Macvie called attention to another possible cause. During the tonic spasm, which may last thirty seconds, there is complete asphyxia; during the clonic spasm there is dyspnœa approaching asphyxia, this lasts

on an average two minutes. Thus for two and a half minutes there is more or less asphyxia; this patient during an hour must have been in an asphyxial condition for about a third of that time. During this partial suffocation the thorax and abdomen are rigid cavities; the inflowing blood from the convulsed extremities soon fills the venæ cavæ, and as there is no relief to the pressure except by the compressibility of the air in the lungs and abdomen or by distention of the superficial veins, the pressure of blood in the veins must be excessive. The pressure prevents the arterial blood passing freely through the capillaries, causes contraction of the arterioles and increased arterial tension. Hence the pulse becomes small, hard, and rapid, the blood is not properly oxygenated, and becomes surcharged with carbonic acid gas. The nerve centres are deprived of oxygen and poisoned with carbonic acid gas, producing the drowsiness, stupor, and coma of the post-convulsive stage. If these effects are multiplied by the number of the fits, their importance will be better understood. How much of these symptoms is to be set down to the asphyxial condition, and how much to some other toxæmic cause? The choice of remedies and the order of their employment would depend on the answer, for if the symptoms are in part due to the asphyxia, then the venous congestion would precede a certain degree of arterial contraction. The indication for treatment in this case would be the neutralising or removal of the excess of carbonic acid gas by oxygen inhalation or relief of venous pressure by bleeding. Purgation as a means of depletion is uncertain in these cases; it is better to bleed first and purge after. The bleeding may be combined with diluent or diluent and excretory treatment. Dr Macvie drew off about nine ounces of blood. This was followed by some immediate relief, and the patient was able to swallow considerable quantities of warm water—the apparatus for hypodermic injection not being at hand. The patient gradually improved during the following days, and began to talk of getting up. Towards evening of the seventh day, however, she became somewhat apathetic. This was more pronounced on the following day. She became stuperose on the ninth day, and died on the tenth. Dr Macvie did not enter into the proximate cause of death, but said he had no doubt as to the eclampsia being cured. The lowered vitality produced by it probably acted unfavourably on a cerebral lesion which had in all probability complicated all four pregnancies.

Professor Simpson read a paper on "Basilysis," and showed an improved model of the instrument. Though the operations requiring these instruments have become more rare, it is all the more necessary that the practitioner should have in his hands

those with which he can most simply and safely effect head reduction and delivery. He discussed the various instruments, preferring those of the cranioclast type to the cephalotribe. That most approved on the continent is the basiotribe of Tarnier, which is possessed of one internal blade and two external ones. Over this instrument the basilyst possesses the advantage of breaking up the base from the inside with the least possible handling, and frequently requiring no aid from the external blade. As illustrating the use and safety of the procedure, Prof. Simpson cited the case of a girl of eighteen years whom he had delivered by basilysis in the Royal Maternity Hospital. She was of stunted and dwarf-like appearance, with dorsal and lumbar scoliosis. The true conjugate was reduced to three inches. The vault was perforated with the basilyst, and the brain and cerebellum washed out with lysol solution. The instrument was then screwed into the base and opened up, when the head almost immediately began to descend. The newer model is lighter and should find more ready acceptance than the older and heavier pattern. The internal and external blades are now made to fit like those of a cranioclast, so that for extraction the basilyst-tractor has now the qualities of what is admittedly the best instrument for delivery of the perforated head.

Professor Simpson also communicated a note on "the use of Levurine." Dr Doyen has recently used it in Paris with much success in all sorts of suppurative conditions. Prof. Simpson gave the results in several cases in which he himself had used it or seen it used. These were certainly encouraging; the cases included tonsillitis, septic conditions after operation or post-partum, and carbuncles. The results seemed to compare favourably with those obtained by use of anti-streptococcic serum. Levurine is the designation given to a dried extract of beer yeast. It may be taken in dram doses thrice daily until the arrest of the suppuration and the reduction of the temperature.

Obituary

Professor Sir Thomas Grainger Stewart, M.D., LL.D., F.R.C.P.Ed., F.R.S.Ed.

By the death of Sir T. Grainger Stewart, Edinburgh has lost a distinguished citizen, the University an able teacher, who not only maintained but advanced its reputation, and the medical profession an illustrious member with a world-wide renown. Sir

Thomas was born at Edinburgh, 23rd September 1837, he died 3rd February 1900. He had a fine presence and a dignified manner, while the genial smile, the warm grasp of the hand, and the cheerful greeting were indicative of the kindly soul within. Time had dealt gently with his external appearance, and his able conduct as president of the British Medical Association at its meeting in Edinburgh in 1898 was evidence that his mental vigour was unabated. Only those who were aware of his increasing difficulty in ascending stairs knew that time was telling on his heart. He was a man of highly strung nervous system and extremely sensitive. Richly endowed by nature, he had highly cultivated his natural gifts. He possessed the power of recognising and rapidly assimilating the important points of a subject, and his memory was not only easily impressed but it was retentive and readily responsive. Doubtless his clinical students remember how he more readily recalled names of places than of persons, and used to address his Infirmary patients by the names of the towns from which they came. He was a good linguist, and was able easily to read foreign medical literature. Last May when attending the meetings of the International Medical Congress in Berlin, his old and honoured teacher, Rudolph Virchow, introduced him as *mein berühmtester Schüler*, and he used to point out to Edinburgh students in Berlin where Stewart sat. The Empress of Germany was so much struck with the ability displayed in his address to the Congress, that she congratulated him heartily when she afterwards saw him at Potsdam.

Sir Thomas' education was specially adapted to fit him for what was the most important work of his life, first in the High School, then in the University of his native city, afterwards in Berlin, Prague, and Vienna. To add to these were his training in the clinical wards of the Royal Infirmary under Laycock and Hughes Bennett; his lecturing, first on materia medica, later on pathology, when he held the appointment of Pathologist to the Royal Infirmary, and still later his appointment as one of the Ordinary Physicians to that institution; all these contributed to his education for the most important part of his life's work.

Both in his systematic lectures and in his clinical teaching he endeavoured to impress upon his students the necessity for a pathological basis for treatment. Most generations of students doubtless remember, in this relation, his anecdote of the lady suffering from indigestion who said to her French physician, "Doctor, you would require to cut open my stomach and see what like it is." He replied, "Ah! madam, I have cut open many stomachs like yours, and I know exactly its appearance." His orderly, systematic habit of thought, his rich command of language, his power of vivid description, fitted him for imparting

PLATE X.—SIR THOMAS GRAINGER STEWART.

to others what he desired to teach; while his impressive manner, interesting style, fluency and readiness of speech enlivened with flashes of wit, rendered his teaching not only powerful but attractive. His natural kindliness of heart drew his pupils to him, and when as an extra-mural lecturer his classes were smaller than in later years, his hospitality to his pupils enabled them to know more of the *man* than was possible to those who attended his University classes.

John Hughes Bennett did much to advance the teaching of medicine, especially clinical medicine, and to render it more practical. Grainger Stewart developed the methods of his master, and he was the first to introduce *tutorial classes* for the teaching of physical examination and diagnosis.

He contributed to the advancement of medical science by his differentiation of waxy disease of the kidneys (which work he did while resident in the wards of the Royal Infirmary), by researches on bronchiectasis, on various kidney conditions, waxy liver, the deep reflexes, peripheral neuritis, albuminuria, by addresses on scarlatina, influenza, tuberculosis, etc. Probably his best known works are that on Bright's disease of the kidney and that on albuminuria.

He had a great enjoyment in life, was a large-hearted man with wide sympathies. A warm-hearted and constant friend, he allowed neither difference in politics nor in church to interfere with his friendship. He was very hospitable, and whether as host or as guest, his genial nature and his brilliant powers of conversation were a great attraction. He was a good *raconteur*, although it was rather difficult to get him to undertake the *rôle*. He was most highly esteemed by those who knew him best, and was seen to best advantage in the love and peace of his home. But he did not take life easily. He often undertook distant journeys in connection with consultation work, early or late, in order that he might meet his class at the appointed hour. He was a busy worker, even when on holiday. He regularly plodded through the home and foreign journals in order to keep abreast of the times, by no means a light task when epitomes of current medical literature were unknown. When confined to bed his mind was ever actively at work, it might be on an article for one of the journals, or on some address, or other work. He found time to dip into the literature of the day; the daily newspaper did not occupy him long, he saved the time for literature of a less ephemeral character. He was interested in Scottish history, and made a special study of that relating to the sixteenth century. He was familiar with the Scottish dialect, could read and use it effectively; he published in the *Spectator* and *Blackwood* very expressive translations into

Scottish verse of some of Heine's poems—*Die Lorelei*, *Mein Kind wir waren Kinder*, etc. An article from his pen on Queen Mary appeared in *Blackwood* some years ago, and his latest effort—a chronicle play, *The Good Regent*—was very favourably criticised by those well qualified to form an opinion. It contains some richly expressive word pictures.

He was a devout Christian. His religion was not a superficial gloss, but the expression of deep conviction. While he ardently supported his own church—and his death is a great loss to that church—he had sympathy with all evangelical bodies. He founded a book fund in connection with the New College, whereby the students could obtain books at a reduced rate: the scheme has proved to be of great advantage. He greatly enjoyed the Bible classes for students which for years were held in his house; he originated the idea of a Medical Students' Christian Association, and the meeting at which its formation was arranged was held in his drawing-room in Queen Street. For six years he was president of the Edinburgh Medical Missionary Society, and as such he was highly esteemed by the other Directors, and his influence and reputation were of great benefit to the Society.

Sir Thomas was liberal both with his means and time. In his giving he was sympathetic and conscientious; much of his charity was of such a kind that it was not known to the public. Nearly every medical man has to perform a large amount of gratis work, some are blamed for the large amount which they do; probably few had such frequent claims made on their time and skill in this way as Sir Thomas had, nevertheless he was ever ready to see patients in consultation without fee when he was informed that the means were small.

During the last weeks of his illness he maintained his interest in outside affairs, and continued to cherish the hope that cardiac compensation might be restored and that the results of backward pressure might be removed. Although those weeks were rendered uneasy by several trying conditions, he preserved a spirit of fortitude, resignation and cheerfulness, and occasionally there was one of those flashes of humour which were characteristic of him.

By his original work Grainger Stewart has done somewhat for the advancement of medicine, but he has contributed much more to the progress of science and to the weal of his fellow-creatures by the kind of education which he has given to many generations of students. His teaching of medicine extended over a period of thirty-five years. When he was appointed to a ward in the infirmary he began clinical teaching, and continued it till last summer. He lectured on medicine for three years in

the Extra-Mural School, and from 1876 till 1899 in the University. His last extra-mural class was attended by over one hundred students, and his university classes were generally three times as large. The number of students, therefore, who have come under the influence of his teaching is large. He implanted in their minds the necessity for system, precision, and scientific methods of examination, and for endeavouring to obtain as far as possible a pathological basis for treatment. Men who had such an education are now in all parts of the globe working for the weal and healing of their fellows.

As a teacher, as a physician both in private practice and in infirmary work, and as a consultant, he obtained well merited success. He was much esteemed and trusted by his patients, and a large number of them ever maintained towards him sentiments of affectionate regard. His reputation is not confined to the British Isles and the Colonies, but extends to the Continent of Europe and America. His ability has been recognised by several universities and other bodies which bestowed upon him various degrees *honoris causâ*, by his having been appointed Physician in Ordinary to the Queen, by his having received the honour of knighthood, and not least among his honours by the distinguished positions which his medical brethren called him to occupy—president of his own college, president of the section of medicine at the annual meeting of the British Medical Association in 1878, president of the Association at the meeting in Edinburgh in 1898. The ability, grace, geniality and dignity with which he performed the duties which then devolved upon him, were subject of general remark.

The secret of his success probably lay not so much in his rich natural endowments, although these without doubt largely contributed to it, as in the high ideal which he had of his object and duty in life, in the fact that he, early in his professional career, had as an objective the chair to which he afterwards attained, and that he ever aimed at taking a foremost place in whatever work he undertook.

By the death of Sir Thomas Grainger Stewart the world is made poorer. Hundreds of his former students in all parts of the world, when they learn that he is no more, will feel a pang of regret, and they will at the same time have flashed upon the screen of memory pictures of pleasant hours spent in Grainger Stewart's lecture-room, or in his wards in the Royal Infirmary of Edinburgh.

J. R.

Editorial Notes and News

The Chair of Practice of Medicine in Edinburgh

The vacancy in the Chair of Practice of Medicine in Edinburgh is arousing much interest in the profession. The filling of any chair is of course a serious responsibility for the patrons, and a matter of moment to the University, while the appointment might materially affect the whole Medical School of Edinburgh. In such a School as Edinburgh, it would be strange indeed were there not many capable, able and distinguished local candidates. Many persons hardly realise the extended and untiring activities of the School, and in Edinburgh itself, it is doubtful if even the workers realise that it is to their individual and united activities that the pre-eminent reputation of Edinburgh as the greatest teaching and clinical School in Britain is due. With reference to the filling of the Chair of Medicine, our readers may be interested to know that there are five members of the Medical Staff of the Royal Infirmary lecturing on Systematic Medicine and delivering regularly complete courses of lectures recognised by the University as qualifying for graduation; three of them have been thus occupied since the years 1877-1879-1880 respectively; other members of the medical staff have been actively engaged in Clinical Medicine since they graduated, and have been systematic lecturers in the School from 1881-1884 and 1885.

The candidates who are already in the field are Dr John Wyllie, Dr Alexander James, Dr Byrom Bramwell, and Dr G. A. Gibson. Of these, Dr Wyllie is the senior, and is a consulting physician to the Royal Infirmary, having completed his allotted years on the active staff a few years ago. Last winter he was appointed to the charge of Professor Fraser's wards in the Infirmary during the Professor's absence in India; this winter he fills Sir Thomas Grainger Stewart's chair, and lectures to the class of practice of medicine. Both now and in the past he has been a popular and successful clinical and systematic lecturer. He is acknowledged to be a man of exceptional brain power, and is the author of a work on *Disorders of Speech*, which occupies an unique position.

Dr James is a full physician to the Infirmary; he has written many monographs and papers on medical subjects; he is a successful teacher, and a very popular member of the staff.

Dr Byrom Bramwell's name is widely known; during many

years his pen has been unequalled in its productiveness; he is an able clinician, and his extensive clinical experience has been recorded with great fulness and detail. He also is a successful clinical and systematic lecturer.

Dr Gibson is a younger man, and is still an assistant physician, but he too is a popular teacher. In addition to many papers, he is joint-author with Dr Russell of a book on *Physical Diagnosis*; he has more recently written a book on *Diseases of the Heart and Aorta*, which is characterised by ability and erudition.

Other names are mentioned, but as they have not so far openly entered the field, we need not refer to them at present.

From the four mentioned, it will be seen that the curators have a rich field from which to select. In some quarters it has been whispered that there might be candidates from outside Edinburgh and Scotland, but the selection of an outsider would be such a blow to the Edinburgh School that the suggestion cannot be entertained. In some departments the choice is properly not confined to Scottish teachers, because of the necessarily limited supply, but in the clinical departments, with their associated systematic teaching, no such condition holds. In Edinburgh there is a wealth of men of ability, culture, eminence, teaching experience and mature judgment, well fitted to fill the vacant chair.

CURLING MATCH.—The annual curling match between the Edinburgh Royal Colleges was played on Saturday, February 10th, at Morningside. The weather was from the curlers' point of view perfect, and the players were very ready for the lunch which Dr Clouston kindly provided. After four hours vigorous curling, victory rested with the Physicians, who are thus enabled to place the Chiene Cup alongside the golf trophy which they won last spring.

The following were the rinks and scores :—

R. C. P.		R. C. S.	
F. D. Boyd.		Russell Wood.	
Norman Walker.		J. W. B. Hodsdon.	
F. W. N. Haultain.		John Chiene.	
A. D. R. Thomson.	—32	F. M. Caddell.	—14
J. Carmichael.		J. Donald.	
R. Lucas.		D. Wallace.	
T. J. Thyne.		J. V. Paterson.	
T. S. Clouston.	—25	R. Kirk.	—19

Majority for the R. C. P., 24 shots.

ROYAL COLLEGE OF PHYSICIANS, EDINBURGH, ROYAL COLLEGE OF SURGEONS, EDINBURGH, AND FACULTY OF PHYSICIANS AND SURGEONS OF GLASGOW.—The quarterly examinations for the Triple Qualification in Edinburgh were concluded on 27th inst., with the following results :—

First Examination, Four Years' Course.—Of nine candidates the following six passed the examination :—Edward Henry Smith, County Cork ; David Charles Williams, Ystradgynlais; Honorine Forbes, Maulmain ; James Levack, Caithness ; Ernest Arthur John Ersser, Aberdeen ; and Churchill Beadon Clarke, Trull, Taunton ; and one passed in the division of Chemistry.

First Examination, Five Years' Course.—Of fifteen candidates the following seven passed the examination :—Alexander Blaxell Hugh Pearce, Edinburgh ; Adam Wilson Hall, Edinburgh ; William Fleming, Spennymoor ; William Sayce Batten, Middlesbrough ; Lionel Pierre-Marie Alexis, Grenada, B.W.I. ; Samuel John Graham, Lisbellaw ; Wilthew Thomas Nicholson, Hexham ; and two passed in Elementary Biology and one in Chemistry.

Second Examination, Four Years' Course.—Of fourteen candidates the following nine passed the examination :—Alexander Cameron Campbell, Glasgow ; William Dale Lawton, Alderley, Cheshire; Henry Edwin Blackwood, Kirkintilloch ; David Charles Williams, Ystradgynlais ; John Archibald Campbell, Inverness-shire (with distinction) ; Thomas Jones Evans, Cardiganshire ; James Marshall Campbell, Banffshire ; William Edward MacManus, Longford ; and Thomas Dow, Crossford.

Second Examination, Five Years' Course.—Of twenty-three candidates the following fourteen passed the examination :—John Joseph Adrien, Drogheda ; Thomas Weir M'Cubbin, Muirkirk ; Mary Lytle Lytle, Belfast ; William Bates, Partick ; John Boyd, Hillsborough ; Robert Patrick Hall Blackstock, Galashiels ; Donald Grant Dingwall, Aultbea, Ross-shire ; Thomas Gibbons, County Mayo ; Stanley Lister George Donald Maclaine, Mountainash ; Roger Llewellyn Williams, Mountainash ; Samuel James Mathewson, County Tyrone ; James MacLean, Inverness ; Henry Doig, Glasgow (with distinction) ; and John William Cross, Houghton-le-Spring ; and five passed in Anatomy.

Third Examination, Five Years' Course.—Of thirty-two candidates the following twenty-one passed the examination :—James Smith Colquhoun, Durham ; James Cotter, County Cork (with distinction) ; Daniel M'Intyre, Littleborough ; Alfred James Hewitt, County Tyrone ; John Douglas James Bruce, Edinburgh ; John Flanagan, Castlerea ; Archibald Buchanan

Laidlaw, Scotland; William Henry Ferguson, Yorkshire; Joseph Crawford, County Monaghan; William Deery, County Tyrone; Robert George Campbell, County Antrim; Charles Henry Farquharson, Jamaica; William Turner, Glasgow; Owen Felix M'Carthy, Cork; Henri Oscar Pomme di Mirimonde, Barnsley; Patrick Carrol, County Cork; Arthur Harold Wilson, Worcestershire; Jeanie Newton, India; John Stewart, Glasgow; Charles Henry Nash, Cork; and Sarah Harper Smith, Aberdeen; and two passed in Anatomy and three in Materia Medica.

Final Examination.—Of ninety-three candidates the following forty-six passed the examination and were admitted L.R.C.P.E., L.R.C.S.E., and L.F.P. & S.G.:—Winifred Hill, Ceylon; Edward Burr, Buenos Ayres; James Wallace Dow, Kinnoull; Walter Asten, Dewsbury; Thomas Duff, Duddingston; John M'Elroy, Perth; George Frederick Beacher, Hemingfield, Yorks; Frederick Jeeves, Belfast; Isabella Anne Watson, Mid-Calder; Marcus Anslow Alabone, London; Alfred James Hewitt, Ireland; Willie Cunliffe, Mytholmroyd; Hilda Maud M'Farlane, Edinburgh; Thomas Stratford Logan, Yorkshire; Selby Woodhouse Morton, New South Wales; Arthur James Morkill, Stranraer; Robert Girdwood, Glasgow; Arthur Ernest Whitehead, Sheffield; Thomas Holmes, Glasgow; Frederick Alexander Georgeson, M.A., Wick; George Frederick Stoney, Tipperary; Francis Peak Maitland, Punjab; Matilda Coutinho, Bombay; Kurien Eapen, Travancore, India; Chittur Kuppaswami Pillai, Madras; John Thomson Kilpatrick Thomson, Glasgow; Charles Augustus Vogwell, Southampton; John Burnett Thorburn, Roxburghshire; George Henry Fullarton Graves, India; William Brown Heagerty, Passage West; Michael Sullivan, Edinburgh; Dudley Jeaffreson, London; John Allison Maclardy, Shrewsbury; William Henry Allan Raffel, Colombo; James Garden Gray, Aberdeen; Christopher Charles Beling, Colombo; Arthur Bryans, Sunderland; Thomas Harrison, Easingwold; James Ebenezer Boon, Kirkcaldy; Charles Frederick Brears, Kellingly, Yorks; George Herbert Leonard Hammerton, Hoyland, Yorks; Chiranjiva, Bharadwaja, Punjab; Thomas Frederick Hillyer Blake, Bournemouth; William Edward Cuthbertson, Monmouthshire; Richard Wilbert Perry, Canada; and Henri Oscar Pomme di Mirimonde, Barnsley; and seven passed in the division of Medicine and Therapeutics, three in the division of Surgery and Surgical Anatomy, eleven in the division of Midwifery, and ten in the division of Medical Jurisprudence.

New Books and New Editions

A Manual of Surgical Treatment. By W. WATSON CHEYNE, M.B., F.R.C.S., F.R.S., etc.; and F. F. BURGHARD, M.D. and M.S. (Lond.), F.R.C.S., etc. In six parts. Part II. 8vo., pp. 382, 141 illustrations. London: Longmans, Green & Co. 1899. Price 14s.

WHEN the first part of this work appeared it was with great satisfaction that we found ourselves able to speak of it in the highest terms of praise. We confidently looked forward to the issue of the subsequent volumes, and our hopes have in no way been disappointed by the one now before us. In a supplementary preface to Part II. the authors have thought it necessary to meet certain criticisms which have been offered on the general scope of the work—criticisms which, in our opinion, can only be classed as captious. The work professes to be a Manual of Surgical Treatment, and a manual of surgical treatment it is. To have added to it chapters on clinical symptoms and pathology would have been to convert it into a System of Surgery, and, speaking for ourselves, we should not have thanked the writers for their pains. We are pleased to find that in the second volume they have adhered to their original plan of giving only the salient features in symptomatology and pathology, questions of treatment alone being discussed in detail. So judiciously has this been done that while, in theory, this work is supplementary to the systematic textbooks, in practice, the reader who consults it on matters of treatment, need seldom go further to learn the essential clinical and pathological data on which the procedures recommended are based.

This part includes all the Deformities usually described in orthopædic surgery except Scoliosis, which, with good reason, we think, is deferred until the surgery of the spine comes under review. The surgical affections of the individual tissues take up the remaining two-thirds of the volume. The first chapter is devoted to the deformities of the fingers and toes, and will prove invaluable to many who have to treat these comparatively trivial, yet exceeding troublesome conditions. We note with interest how solicitous the authors are for "hammer toes," the

amputation of which "is not at all to be recommended." We think, too, that they lay undue stress on the errors of the boot-maker in the etiology of hallux valgus. It appears to us that if this deformity was "essentially produced" by the wearing of narrow boots, few would escape. That pointed boots aggravate the deformity when it appears, as well as the discomfort it produces, there can be no doubt, hence they are justified in insisting upon an anatomical model of boot for those who suffer.

The association of flat foot with hallux rigidus is duly emphasised; and when an operation is called for to correct the condition of the toe, the excision of the base of the first phalanx is recommended.

In the last paragraph on the treatment of Dupuytren's contraction the writers are unfortunate in using the term "tenotomy" to indicate the sub-cutaneous division of the palmar fascia. As it stands it is apt to give the reader the impression that the tendon is to be divided.

Flat foot is ably discussed in the second chapter, due emphasis being laid on the value of systematic exercises in early cases; while Whitman's spring is highly recommended when an artificial instep is indicated.

The treatment of the different varieties of club foot is fully described, and we are glad to find that all forms of complicated apparatus are conspicuous by their absence.

Among the other deformities of the limbs, coxa-vara and congenital dislocation of the hip are described in the light of modern knowledge of the treatment of these conditions.

Of the chapters on individual tissues the most excellent are those on the lymphatic glands, the tendon sheaths [although we do not quite follow the rationale of injecting tincture of iodine into the tendon sheaths in cases of simple chronic teno-synovitis (p. 219)], and the nerves. In writing this last chapter the authors appear to have overlooked Kennedy's recent work on the regeneration of nerves.

The surgery of the blood-vessels is fully discussed; the operation for ligature of each of the arteries being minutely described.

The illustrations throughout the volume, although simple, are admirably calculated to fulfil their object.

We would express the hope that the higher calls at present

being made upon the time and energies of the senior author of this work may not materially delay the appearance of the remaining volumes.

Skiagraphic Atlas of Fractures and Dislocations, with Notes on Treatment for the Use of Students. By DONALD J. MACINTOSH, M.B., Medical Superintendent, Western Infirmary, Glasgow. 4to, 80 plates. London: H. K. Lewis. 1899. Price 12s. 6d. net.

THE intense interest aroused, both in scientific circles and amongst the laity, by Professor Roentgen's discovery, and the high hopes formed as to its value for surgical purposes, makes us welcome such a volume as that before us. In it we have pictorially recorded the experience of one who has devoted much time and attention to the subject, and we are able to form some idea of the utility of skiagraphy in at least one department of surgery.

This atlas is intended for the use of students, but we fear the author has, by certain omissions, neither given himself justice, nor the student a fair chance of deriving that amount of benefit from the work which is inherent to it. For it requires not only considerable practice, but also some guidance to intelligently examine and correctly interpret the showings of a skiagram. Whether the author has been justified in tacitly assuming that the students have had the former, and can therefore dispense with the latter, we much doubt. We fancy the average student will share with us the difficulty of diagnosing not a few of the lesions depicted without reference to the letterpress, a difficulty which arises largely from the want of a normal standard. It is true that the author has introduced illustrations of normal parts at different places, but they are all unsatisfactory. A hand is not in its normal state when covered by buttons, beads, and bangles. Like us, the student will probably feel that he would have learned more from the plates had they been accompanied by a brief clinical description of the case with special reference to the difficulties met with in diagnosis, and the aid obtained from the skiagram. A statement of the side of the body affected, the age of the

patient, and the position of the limb while being photographed, would also have added to the educational value of the plates.

Space for these details might well have been obtained by the omission of the "Notes on Treatment," which, however clear and accurate they may be, seem to us somewhat out of place in their present form.

Taking the work as we find it, however, there is much to be learned from it, although we confess that we do not always see in the skiagram what the description tells us we ought to see. Plate 16, for example, does not convince us that "the coronoid process and upper end of the shaft of the ulna are broken off." We see precisely the same appearance in Plate 28, where no such lesion is suspected. In Plate 17, too, we see the fracture of the ulna, but not that of the radius; and in others we have to exercise some faith before we can see what is expected of us.

Six cases of Colles' Fracture are figured, and bring out the interesting fact—that separation of the styloid process of the ulna is a common complication of this injury. In five of the cases the lesion is present, and in the sixth (Plate 8) something very like it is seen. The equality in level of the two styloids is also clearly brought out in these plates.

Another instructive group are the thirteen plates illustrating injuries in the region of the elbow joint (Plates 18 to 30), which, like those of lesions about the ankle, are all clear and convincing.

In addition to the plates of fractures and dislocations, the author has added a series of seventeen illustrations of such conditions as coxa-vara, foreign bodies in different situations, tumours and tubercular lesions of bones, congenital deformities and pulmonary osteo-arthropathy, all of which are most instructive.

If we have ventured to indicate points which appear to us of the nature of defects in this work, it is because we feel somewhat disappointed, not to say irritated, that the author has missed the opportunity of making his excellent and valuable collection of plates more instructive for want of a fuller and more detailed account of the cases he has illustrated.

The work is well printed and bound, and forms a handsome contribution to the literature of surgical skiagraphy.

Yellow Fever in the West Indies. By IZETT ANDERSON, M.D., Edin. Crown 8vo, pp. 106. London: H. K. Lewis. Price 3s. 6d.

DR ANDERSON'S unambitious monograph is, as he claims for it, a clinical study, and an aid to those entering on the practice of medicine in the West Indies.

After pointing out that negroes and other coloured persons seldom suffer, that second attacks are rare, and that immunity is secured to Europeans in direct ratio to length of residence, he dwells on the constant presence of albumin in the urine in attacks of yellow fever, and points out the prognostic value of diminution of albumin in the course of the disease. He recommends the abortive treatment at the outset of the disease, and considers it was frequently successful during a widespread trial in Demerara. It consists in giving a scruple of calomel and 24 grs. of quinine well mixed together, and this, in severe cases, repeated after three or four hours, and followed by a saline purgative.

Useful hints are given as to diet, nursing, and medicinal treatment. Dr Anderson, in common with the great majority of observers, holds strongly the opinion that yellow fever is not contagious. In his thirty-four years' experience he never saw a case where contagion was even probable; he quotes cases where contagion seems to have been impossible, and he points out that removal (say, of a company of soldiers) from an infected locality immediately checks the spread of yellow fever. He regards it as essentially topical.

"I do not believe," he says, "that the very worst conditions of sanitary neglect will ever by themselves alone generate a case of yellow fever. Something else must be added thereto. What that additional factor is we know not; when it is present, however, the disease will undoubtedly spread most rapidly where insanitary conditions exist."

Mental Affections: An Introduction to the Study of Insanity.
By JOHN MACPHERSON, M.D., F.R.C.P.Ed. Demy 8vo.
London: Macmillan & Co., Ltd. 1899. 12s net.

DR MACPHERSON is to be congratulated upon his having produced an excellent work on one of the most difficult specialties in medicine. It is characterised by a lofty, broad, and philosophic treatment of the subject; by a freshness of view, especially in the matter of classification, which stamps the book with an individuality of its own, and should gain for it a place amongst the leading English works upon insanity; and lastly, by the evidence of a strong inspiration from the continental schools, especially the French but also the Italian and German, with their doctrines of degeneration and the toxic origin of the insanities. While the volume is described by the author as an introduction to the study of insanity, it is so only in the sense that it is not exhaustive in detail, and from the high level from which it is written it appeals more particularly to senior students and practitioners and to those specially interested in neurology and psychology.

Part I. is "Introductory" and deals with matters etiological, under the headings of morbid heredity, the evolution of insanity, and the determining causes of insanity. In evidence of the wide and liberal views which the author takes of his subject, we may instance his attitude towards the important factor of heredity. Pathological heredity is as real a fact as physiological heredity, but with this difference, that it is usually not the positive pathological characters (physical and mental) which are transmitted, but a vital functional or nutritive defect which predisposes to disease and may under unfavourable environment engender diseases of very different appearances. This inherited defect is always associated with a corresponding change in some function of the nervous system, this change in nervous function being called degeneration or neuropathia. Morbid heredity therefore always means in the parents an inability to transmit perfectly normal characters, physical and nervous, and in the offspring a vice of organisation or degeneration. The co-relation of the neurotic with the physical or nutritive element in degeneration prepares us for the author's tenet of the transmutability of the diatheses and neuroses in their hereditary transmission. "If we can establish in any of our patients a family history of these affections (the diatheses and neuroses), the actual presence of insanity, which is merely a neurotic manifestation, is by no means necessary. Is there a history of gout, rheumatism, diabetes, gravel, phthisis, hysteria, migraine, epilepsy, asthma?

If so, your patient supplies the instance of transformed neurosis: the morbid taint is fully established" (p. 22). It naturally follows that in the author's opinion the ideally normal man, the man without any hereditary predisposition, the man with no diathetic or neurotic taint in his family or ancestral history, cannot become insane. The reflection rather arises that probably no such man exists, and that any man under sufficiently abnormal circumstances may become diathetic or neurotic or even insane. The causes of degeneration and its various diathetic, neurotic, and teratological manifestations are sketched in a very suggestive manner.

Two chapters are devoted to the determining causes of insanity, of which the toxic engages more attention than all the others together, being considered under the headings of auto-intoxications, microbic infections, and poisonings by chemical substances. As showing how the causes of insanity may overlap one another in their mode of operation, we may refer to one of the experiments of Charrin and Roger which is quoted in favour of the view that in nervous shock from severe physical or psychical causes, microbic poisoning may play an important part. A rabbit whose blood was free from microbes was rotated on a revolving table for four hours, and on being examined immediately afterwards was found to have its blood so rich in bacteria that from one drop 800 colonies were developed. Hence it is suggested that in severe nervous shock there is temporary suspension of the function of brain trophesis and probably also of phagocytosis, with consequent invasion of the system by bacteria.

Part II. is "Physiological and Pathological." One chapter is devoted to the histology and physiology of the brain cortex, and refers to the modern ideas of cortical lamination and localisation and the doctrine of the neuron. Another chapter deals with normal psychology, and emphasizes the importance of the operations of the "Unconscious Mind." Mind is no longer, as in the older psychology, to be regarded as co-extensive and synonymous with consciousness. On the contrary, the greater bulk of our mental processes do not enter consciousness, which is but an incidental quality of mind more or less co-extensive and synonymous with attention. A thoughtful exposition is given of other qualities of the "Conscious Mind," such as sensation, ideation, memory, emotion, action, will, and personality. Two chapters follow on pathological symptomatology. One of these contains a description of the stigmata of degeneration, bodily and mental. The other, on "cerebral dissociation," is an attempt at an explanation, on a theory of morbid psychology and physiology as applied to the neurons of

the ideational and other centres, of the pathogenesis of the grand symptoms of mental disease: melancholia, mania, confusion, delusion, hallucination, etc. The main hypothesis is that any group of neurons may have its neural excitability and conductivity diminished or increased, that diminution of excitability, or in other words, undue resistance to the flow of impressions is the condition in melancholia, and that increase of excitability or undue permeability to impressions is the state of the neurons in mania. Many facts, clinical and experimental, are adduced in favour of this hypothesis, which is ably worked out by the author for the other cardinal symptoms of insanity.

Part III., on "Clinical Symptomatology," deals with the different varieties of insanity, their clinical description, etiology, pathology, and treatment. The classification adopted is original, and although the author's views at times are somewhat sweeping and involve the acceptation of much that has not yet been proved, they are always suggestive and indicate wide fields of future research for the student of mental disease. The classification is not a logical one, as is seen from the primary grouping of the insanities into the five main classes of the idiopathic and confusional insanities, and the insanities of the degenerate, of the neuroses, and of organic brain diseases.

The Idiopathic Insanities, or those arising as a rule without any apparent external cause and depending upon some underlying morbid condition of the nervous system, comprise mania, melancholia, and circular insanity, which are essentially periodic, and progressive systematised insanity (paranoia). The description of the last, on which Dr Macpherson is the recognised authority in this country, is able, clear, and concise.

The Confusional or Toxic Insanities form a large class brought together on account of an asserted clinical similarity characterised by a symptomatology of mental confusion and physical debility, and possibly explainable on a toxic basis. The confusional insanities fall into three groups: (1) Forms due to shock, or exhaustion, or malnutrition, comprising delirium of collapse, acute primary mental confusion, and stupor. (2) Forms due to auto-intoxication or microbic infection, including cretinism, myxœdema, general paralysis, delirious mania, puerperal and post-febrile insanity. The author inclines to the view that general paralysis is a parasyphilitic disease, but we do not subscribe to its inclusion amongst the toxic insanities. (3) Forms due to voluntary intoxication, comprising alcoholism, morphinism, cocainism, and chloralism.

The Insanities of the Degenerate comprise idiocy and imbecility, premature dementia, moral insanity, communicated insanity, and the special forms of "episodic syndromes" (*e.g.*

obsessions, phobias, impulses, perversions, etc.), and of systematised delusions which are met with in the more profoundly degenerate as compared with mild degenerates.

The Insanities connected with the Neuroses, hysteria, epilepsy and chorea are next described, and the author thinks there are good grounds for regarding them in the light of auto-intoxications.

The Insanities caused by Brain Diseases include those of apoplexy, brain tumour, brain syphilis, traumatism, and sunstroke, senile decay, and secondary dementia.

A chapter on medico-legal matters brings to a close a work which greatly enhances the author's reputation as physician and alienist, and which should be in the possession of every earnest student of mental disease. The author's style withal is fluent and graceful, and this, combined with the clear type and good taste of the publishers, makes the volume extremely pleasant reading.

Clinical Studies in Vice and in Insanity. By GEORGE R. WILSON, M.D., Medical Superintendent, Mavisbank Asylum. Demy 8vo, pp. 234. Edinburgh: W. F. Clay. 1899. 7s. 6d. net.

DR WILSON presents to us in this volume a continuation of his studies on drunkenness, and also the results of his observations upon other vices and upon the insanities.

Part I. of the book is devoted to the pathology and treatment of alcoholism. According to the author alcoholism is essentially a disease of the nervous system, being variously manifested by all the nervous affections attributable to alcoholic excess, such as drunkenness and the alcoholic forms of insanity, epilepsy and paralysis. The lesions found in other viscera are regarded rather in the light of complications than as essential parts of the disease, though doubtless they are predisposed to by an alcoholic impairment of trophic function. Alcoholism, of which drunkenness or inability to control the drinking habit constitutes the first stage, consists essentially in a progressive dissolution of nervous function from above downwards, the result being a reduction of character to a state comparable to puerility or childishness. The author's view of the nature of the disease is arrived at not by the anatomical and chemical methods of the pathologist, but by inference from carefully

observed clinical facts explained in the light of modern nerve physiology. The pathology of alcoholism is thus considered from the point of view of its dynamical pathogenesis, at the early stage, when alcohol in excess is beginning to produce its morbid effects.

Dr Wilson emphasises the views of Foster and other physiologists as to the existence of a constant stream of nerve energy passing from the periphery inwards to the nerve centres, and from the nerve centres outwards to the periphery, and serving as the normal stimulus for the maintenance of healthy nerve metabolism or tone. This stream of nerve energy may be as truly spoken of as a "circulation" as that of the blood, and the function of the "nerve-circulation" is paramount in the brain cortex.

The first effect of alcohol in excess is to produce an interference or obstruction of the nerve-circulation, that is, a trophic reduction of nerve excitability and nerve conductivity; in Dr Wilson's words, a "tonic" depression or degeneration. This lesion begins in the highest dendritic systems of the cortex—those which subserve the supreme or purposive functions of the mind, and it spreads progressively along trophic lines throughout the rest of the nervous system. Attention is drawn to the importance of Dr Hughlings Jackson's distinction between primary and secondary lesions in progressive dissolutions of the nervous system, the primary occasioning impairment of function with symptoms of negative character which are apt to be masked by the more positive but less essential secondary symptoms that result from the less controlled working of the mechanisms "below" the primary lesion. In alcoholism the primary and essential symptom and the one which calls for treatment throughout the disease, is impairment of the supreme mental function of purposive self-direction in its double aspect of initiative and discipline. Dr Wilson points out that discipline is soonest regained by conforming to the regularity of life in an asylum or retreat, but that initiative is best fostered by a judiciously graduated return to the outside world, where the incentives to work and interests of life in general are more powerful.

Part II. consists of a clinical description of twelve cases of alcoholism, arranged in three groups—the high-class, the

average, and the degenerate alcoholists. The special object of this investigation is to ascertain what manner of men and women become drunkards. The author is unable to prove the existence of any characteristic alcoholic temperament or diathesis, "the average drunkards, who are in a large majority, being neither more weak-minded nor more strong-minded than their neighbours."

Part III. consists of a series of clinical studies of fifteen cases of insanity, associated mainly with the periods of puberty and adolescence, the climacteric and senility, but showing the importance of the spiritual factor in the production and alleviation of mental disease.

In his "clinical studies" Dr Wilson exhibits keen powers of observation, especially for the facts of mental life, a happy imagination and a distinct gift for the analysis of human character. Many of the studies are made the text of a thoughtful homily upon important questions in etiology and therapeutics, and in his treatment of these matters the author is always bold and original and not infrequently heterodox. Dr Wilson is specially to be congratulated upon the style of his writing, which is clear and spirited, lively, dramatic and humorous. Many of the "Studies" indeed read like short plays or novelettes. The volume is dedicated by the author to his former teacher and chief, Dr Clouston.

Diseases of the Eye. By G. E. DE SCHWEINITZ, Professor of Ophthalmology, Philadelphia Polyclinic. Third edition. Pp. 696. London: Saunders & Rebman. Philadelphia, 1899. Price 24s.

THE third edition of this favourite American text-book of ophthalmology has been thoroughly revised, and its value enhanced by the introduction of new matter and the addition of fresh illustrations. Particular attention has been given in this edition to the important relation which micro-organisms bear to many ocular affections, especially those of the conjunctiva and cornea. Special paragraphs appear for the first time in this text-book dealing with somewhat uncommon conditions, such as oyster-shucker's keratitis, fugacious periodic episcleritis, etc.,

while the new local anæsthetics eucain and holocain receive attention. The chapters dealing with *muscular insufficiencies* and operations have been largely rewritten and materially changed.

This work made its first appearance in 1892. It very soon obtained considerable popularity in the States and Canada as a practical and reliable treatise on ophthalmology, gaining for its author a leading place amongst the most celebrated ophthalmologists. The appearance of the third edition in the comparatively short space of seven years is in itself sufficient proof of its continued popularity and usefulness, and the author is to be congratulated on his now fully fledged literary production.

Written in a clear, lucid, and almost racy manner, the book presents the subject matter to the student in an easily assimilable form, without burdening the memory with too many theoretical and pathological details. It may indeed be described as a real clinical handbook, in which such important subjects as differential diagnosis and treatment receive careful attention. Well selected woodcuts, chiefly, however, from the works of other authors, are employed to assist the descriptions given of the various diseases, but we find that these have in many instances lost somewhat in the transfer, which detracts from the high standard adopted in the work as a whole.

Throughout the author has studiously avoided controversial subjects, believing, and rightly so, that they are outwith the province of a text-book intended principally for students. There are, however, a few points of clinical importance on which we decidedly differ from the author. He depreciates the employment of cocain in corneal ulceration, even when of phlycenular origin. In our opinion cocain, when employed in the form of ointment, and in conjunction with other well-known remedies, is one of the most valuable therapeutic agents we possess in corneal ulceration. Sulphate of atropine is used on pp. 277, along with vaseline, although it is insoluble in all forms of fats and oils. Sweet's localiser or method of localising may be good, but it is far behind Mackenzie Davidson's. The expression "insufficiencies of the internal recti" is surely not in keeping with the high character of the work.

We can confidently recommend the book to students of ophthalmology as an excellent clinical treatise.

Mentally Defective Children: their Treatment and Training. By G. E. SHUTTLEWORTH, B.A., M.D., etc. Second edition. Crown 8vo, pp. 180. Illustrations. London: H. K. Lewis. Price 5s. net.

THE claims of mentally deficient children on the attention of the medical profession as well as on that of the community at large are receiving increasing attention. It is certainly well that this should be so, and we are glad to welcome a second edition of Dr Shuttleworth's admirable little book. The medical man in general practice constantly meets with idiot and imbecile children, and it is very important that he should be able to recognise their condition as early as possible. It is also most desirable that he should know something of the more probable causes of mental defect, and be able to give an approximately correct prognosis and to advise as to home care, education and discipline, even before the question of removal to a special institution arises. We know of no small book which gives this sort of information so well as the one before us.

The pathological classification, etiology, diagnosis and prognosis of mental deficiency are shortly and interestingly dealt with, while more space is given to the consideration of the treatment—general, medical, educational and moral. In this edition we find also a considerable amount of new matter dealing with "those less definite cases of mental infirmity which, in later years, have been described under the specific designation of 'Feeble-minded.'" This class includes children who, although they do not deserve to be called imbecile, are yet of too limited mental attainments to be able to compete with normal boys and girls. The attention of the Education Department was called to these children a few years ago, mainly owing to Dr Francis Warner's very extensive investigations, and Dr Shuttleworth gives the recommendations of the committee which was formed to consider the subject of their separate and special teaching. As such children are found to form about 1 per cent. of the school-going population, their proper treatment is a practically important as well as interesting question.

Although a foremost authority on the scientific as well as on the educational aspects of the subject, Dr Shuttleworth writes

so clearly and simply that his book will be found very useful by intelligent parents and teachers as well as by medical men.

Medical Gymnastics, including the Schott (Nauheim) Movements, being a Text-book of Massage and Mechanical Therapeutics generally. By AXEL. V. GRAFSTROM, B.Sc., M.D. 8vo, pp. 139, with eleven pen and ink sketches by the author. London: The Scientific Press, Ltd. 1899. Price 2s. 6d.

IN his preface, Dr Grafstrom declares the aim of the book to be that of presenting "the subject of Mechano-Therapeutics in a condensed form and in a rational and popular way, in order to make it easily understood by the student." One thing at all events he has attained, and that is the condensation. About one-half of the book is given to the chapters upon Massage and Medical Gymnastics, and the other half is devoted to the consideration of the treatment of various diseases by mechanical methods. The former part is a clear, if somewhat brief exposition of the subject; but the latter is not so satisfactory. When we find Schultze's and Sylvester's methods described under the heading of mechano-therapeutics of infancy, and stretching the os given as the treatment of Hyperemesis Gravidarum, we wonder why version is left out and the manipulations for reducing fractures omitted. Again there is considerable disproportion in the prominence given to various subjects; Nauheim movements are summarised in two pages, whereas floating kidney has seven pages allotted to it. Moreover, the incorporation of articles contributed to different journals only results in a considerable repetition of general facts. Finally, the proof reading might have been done with greater care, especially in a "popular" text-book. The "man in the street" is likely to be mystified by the statement that in the treatment of consumption, "Tuberculosis and the various preparations of anti-tubercle serum have not so far given any brilliant results as curative agents."

The Pathological Statistics of Insanity. By FRANCIS O. SIMPSON, L.R.C.P., M.R.C.S., Assistant Medical Officer, Lancashire County Asylum, Rainhill. Royal 8vo, pp. 140. London: Baillière, Tindall & Cox. 1900. 10s. 6d. net.

THIS volume is the outcome of a suggestion made by Dr Bevan Lewis to the author, while he was acting pathologist at Wakefield Asylum, to compile annually the statistics of the naked-eye appearances of insane brains as examined in the *post-mortem* room. The method of collecting the statistics is first described, and an elaborate scheme is submitted for the ready tabulation of any of the *post-mortem* appearances, normal or abnormal, which are likely to be met with in the scalp, skull, brain and spinal cord. The naked-eye characters of the insane cranium, of the insane brain in general, and of the insane brain in the different varieties of mental disease, are described on the basis of these statistics. Very few generalisations are arrived at, the text consisting mainly of pages of tables and figures in percentages, the calculation of which alone must have been no small labour. The statistics are based upon about 400 *post-mortem* examinations performed at Wakefield Asylum during the years 1894, 1895 and 1896. As to their value in elucidating the pathology of insanity some doubt may be expressed, for they refer only to gross macroscopic appearances. Still they form a useful basis for reference, the tables are a monument of the author's industry, and the book as a whole is an expression of the zeal with which the pathology of insanity is now being studied in our asylums.

A Medical Pocket Dictionary (German, English and French). with supplement containing comparative tables of weights and temperature. By Dr EUGEN LEHFELDT. Second Edition. Berlin: S. Karger. 1899.

A HANDY little book which will be useful to those who have to look up references in languages unfamiliar to them. Like all similar productions, it occasionally leaves undone those words which it ought to have translated, and now and then it translates words which certainly might be left alone. Some English words appear which are only *heard* in the most impolite society,

and only seen in slang dictionaries. The words "*desudation*" and "detruncate" are not words likely to be frequently met with in English medical works, yet here they are with their German and French equivalents (?). When the publisher is preparing his next edition, he will do well to have the English section "revised in England," for it is rather too evidently "made in Germany."

Fevers and Infectious Diseases: their Nursing and Practical Management. By WILLIAM HARDING, M.D.Ed., M.R.C.P. Lond. Pp. 88. London: The Scientific Press, Limited. 1897. Price 1s.

THIS little book aims at giving to nurses such information as shall help them in the observation and care of their cases of infectious disease. It treats of infection and of the various therapeutic and precautionary measures taken against it; of disinfectants and their practical application; and of the nursing of the commoner forms of infectious illness.

The information given is, of course, very small in amount, owing to the size of the book. It is, however, accurate and expressed in plain language; and the directions given are of an eminently practical nature.

The book seems to us a most useful one, and we have great pleasure in recommending it.

On the Prevention of Eye Accidents Occurring in Trades By SIMEON SNELL. London: Bale, Son & Danielsson. Price 1s. net.

THIS little book, consisting of thirty-two pages, is a reprint of the presidential address delivered in the section of ophthalmology at the annual meeting of the British Medical Association last year. It contains many interesting and valuable suggestions as to prophylaxis, which, if adopted, would diminish to a considerable degree traumatic affections of the eye. It will repay careful perusal.

Constipation and its Modern Treatment. By GEORGE HERSCHELL, M.D. Second Edition. Post 8vo, pp. 91. London: Henry J. Glaisher. 1899. Price 2s. 6d. net.

THIS small monograph is the reprint of a paper, printed in large type, and with an index to elevate it to the dignity of a book. The author advocates the treatment of constipation, not by drugs, but by lavage, enemata, massage, exercise, and electricity. There is little that is new, but the views expressed are sound as far as they go. The apparatus figured for washing out the colon may be found useful.

The American Year-Book of Medicine and Surgery. Edited by GEORGE M. GOULD, M.D. Imp. 8vo, nearly 1200 pp., with coloured plates and other illustrations. London: The Rebman Publishing Co., Ltd. 1899. Price 38s.

THE American Year-Book is a huge volume, the product of many workers. It is a laudable effort to give a condensed record of the year's progress in medicine and surgery. The task is enormous. The volume might be of greater value if the digests were selected in a more critical spirit, and the selected articles more fully epitomised. On the present basis the abstracts seem at times of a rather scrappy character.

The Hæmorrhages of Pregnancy, Parturition, and the Puerperium. The Treatment of Puerperal Eclampsia by Saline Infusions, with Notes of Seventeen Cases. By ROBERT JARDINE, M.D., Edin., etc. 8vo, pp. 76, with two plates. Edinburgh: William F. Clay. 1899. Price 1s. 6d.

IN this small book Dr Jardine has gathered together five lectures given at the Glasgow Maternity Hospital, and one paper read before the Edinburgh Obstetrical Society. They must all be well known to the readers of this journal as they have all passed through its pages, but some of us may be glad to have them in this separate form for reference.

Recent Literature

CRITICAL SUMMARIES AND ABSTRACTS

CRANIAL SURGERY

By CHARLES W. CATHCART, F.R.C.S.,
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Some Recent Instruments and their Application.—From time immemorial the skull seems to have been considered a legitimate field for surgical interference, for even pre-historic skulls have been found perforated with holes which have been made apparently deliberately and carefully, and long before death. What these holes were bored for we of course do not know, but it is supposed that they were intended to allow of the escape of evil spirits. In later years similar holes have been made with the object of giving exit, not to spirits, but to fluids and solids confined within the cranial cavity and pressing injuriously on the brain. So far as the effect upon the skull is concerned, however, the result of such operations has always been the same; a permanent gap has been left, because neither the peri-cranium nor the dura mater have the bone-forming properties of peri-osteum, and the amount possessed by the diploe is only very limited. On this account, at a time when other departments of surgery reaped the full advantages of anæsthesia and antiseptics, that of brain surgery was obliged to accommodate itself to the comparatively limited amount of bone which might be removed from the skull without serious inconvenience. Efforts were made to fill up the larger gaps in the skull by bone grafting, or by the implantation of plates of metal between the scalp and the dura mater, but with only partial success. In 1889, however, Wagner[1] showed that it was possible to turn aside a large portion of the skull, and after operating upon the parts beneath, to replace the bone without preventing its subsequent growth. The name "osteo-plastic" is often applied to this method, and its essential feature consists in ensuring that the peri-cranium and scalp lying over the resected piece of bone shall not be detached from it and shall have a broad base continuous with the adjacent soft parts, so that the vascular supply of the whole flap shall be maintained. Although the only channel through which the nutrition can at first be kept up in the bone is the peri-cranium, that has been found sufficient. At the end of the operation, the flap composed of bone and soft parts is replaced and grows again in its old position, so that the continuity of the skull is re-established. When this operation was first introduced, the instruments em-

ployed were the familiar gouges, chisels, saws, and trephines used in surgery, modified only to the extent necessary to meet the requirements of antiseptic practice. By degrees, however, other instruments have been found more suitable for the new operation, and it may be of service if we try to point out some of these which seem most likely to help in extending the application of this method of operating. We shall not include electro-motors working circular saws or drills, or any other pieces of apparatus which are at present expensive or complicated. Such instruments may or may not serve the purpose as well as those which are moderate in price and simple in construction, but it is only the latter which will make an operation popular throughout the country. Within these requirements, two instruments—Braatz's drill and Gigli's wire saw —have been recently recommended for osteo-plastic skull operations, and they seem to deserve wider recognition than they have as yet received.

Before cutting free the bone which is to form part of an osteo-plastic flap, its base at least, and sometimes also various parts of its circumference, are marked out by the formation of apertures in the skull. These may vary in number from two to six or seven according to the method of operating employed, and as it is important to save time as much as possible, the best and simplest way of making these preliminary holes requires, like other details, to be carefully considered. There are practically three kinds of instruments which may be used for the purpose: a gouge, which was used by Wagner; a small trephine, which until recently was adopted by the majority of those who followed him; or a large diamond-shaped drill and burr, which are now growing in favour as simpler and more rapid for the small apertures required. Their diameter need only be about a quarter of an inch or less. Of course by the use of the drill the substance of the bone is lost; but when the main piece which forms the flap has been replaced, drainage holes are required, and deficiencies in the skull of such small size, besides being partly filled up from the diploe, are not of much importance. If we decide then in favour of the drill point for piercing these holes, we have next to consider the best means of causing it rapidly to revolve. There are three different hand instruments used for this purpose in surgery. They have been adopted with the necessary slight modifications from the carpenter's and ironsmith's shop, and are (1) the Archimedean drill, where the handle is forced alternately up and down a long screwed shaft; (2) the bevelled wheel action drill, sometimes known as Collin's drill, where a larger cogged wheel with a horizontal axis is made to work at right angles upon a smaller cogged wheel with

a vertical axis on to which the drill fits; the handle placed upon the larger of the two wheels is turned like the handle of a barrel organ; and (3) the old-fashioned trepan, which works like a carpenter's brace and carries either a drill or the trephine, with which we are most accustomed to see it armed. This is the instrument which has been modified by Braatz [2] so as to render it more serviceable for the operation which we are at present considering. Instead of the crank being turned directly by the hand, it is acted upon through the medium of a small handle. This seems a very slight modification, but it carries with it this great advantage, that the instrument is reduced very much in size without losing its efficiency. The surgeon's hands are brought closer together, and he has much more control over the drill point thus impelled than when a full-sized trepan is used, while the instrument itself can be easily boiled and is light and portable. Another advantage claimed by Braatz is that the rotating movement produced by the surgeon's right hand does not affect the pressure exerted upon the point by his left hand, which cannot be said either of Collin's or of the Archimedean drill. When either of these two instruments is being used, the right hand producing rotation alternately increases and diminishes the downward pressure exerted upon the point with the left hand, and thus delicate work is more difficult. There are, moreover, in Braatz's drilling machine fewer parts to be cleaned, and less loss of power from friction than is the case with either of the other two forms. If so wished, the crown of a small trephine can be substituted for the drill. In every way, therefore, this simple modification by Braatz of the old-fashioned trepan seems to be the best form of hand-drilling instrument that we have for modern surgical practice.

These drilling operations, however, are merely preliminary to the more important step of isolating from the rest a considerable portion of the cranial vault without detaching its over-lying soft parts. To understand the possible ways of doing this we may suppose that the flap is to be a square one. On three sides of the square the soft parts are divided freely, and the bone which is thus exposed can be attacked without difficulty, but on the fourth side the bone must be divided while the soft parts over it retain their continuity with the rest of the scalp. This, of course, puts certain of the otherwise possible methods of cutting through the bone out of the question. Let us, however, consider the exposed portion first. The line of division should be as fine as possible so as to facilitate reunion, and the portion lifted up should be bevelled at the expense of the inner table so that it cannot be forced down upon the brain when it is put back into position. Wagner used a chisel and hammer to cut

through the bone, and carefully avoided removing some splinters, accidentally left after the main piece had been raised, in order that they might guard against its being pressed down upon the brain when replaced. Most surgeons who have followed him have used some form of saw, either small circular saws driven by electro- or hand-motors through a dentist's flexible cord, or fine hand-saws. Cotterill, who has introduced this operation into Edinburgh, and who has made many instructive experiments on the dead subject in connection with it, has had a number of successful cases. He has lately used a fine Hey's saw for this part of the operation, and his results have been most satisfactory. The work, however, is difficult and somewhat tedious when done in this way. Van Arsdale's circular saw[3], driven by an electro-motor, has been highly praised by Thomas of Cardiff and others, but its expense will prove an obstacle to its general adoption. Fortunately, however, in Gigli's wire saw we have an instrument which is not only simple and cheap but which does its work as rapidly as the most expensive form of circular saw driven by electricity.

The idea of using a flexible saw in surgery is by no means new. Chain saws have been sold by instrument makers for probably more than a century, although they have never been very popular. It often happens, however, that an improved method of carrying out an old idea makes all the difference in the world in the adaptation of the idea to practice. Thus Gigli's piece of roughened steel wire, cutting all round, represents an immense improvement on the many-jointed, and relatively cumbrous chain saw of old which cuts on one side only. The exact size, temper, and mode of roughening the wire are of great practical importance, and much credit is due to Gigli and the instrument maker G. Haertel, who have worked out the necessary details between them. We need not, however, allude to the instrument further than to say that it consists of a piece of steel wire of a certain temper and roughened in a particular way, the ends being softer and provided with loops into which a handle can be conveniently hooked. The wire is passed behind the bone to be divided, and is drawn by a see-saw movement backwards and forwards against it. Although a very insignificant-looking implement, it will cut through the hardest bone in the body with an ease and rapidity which is most surprising to any one who sees it for the first time. It will be evident from this indication of its mode of use, that before being applied to the skull the wire saw must first be introduced between the dura mater and the bone. Gigli himself did not think of applying the saw to the skull. His first account of the saw was published in 1894,[4] and in 1897,[5] when he

published certain important improvements in its manufacture and indicated the uses to which it had been applied, he incidentally mentioned that the bone operations for which he thought it was *not* applicable were those on the skull ("Knochenoperationen, abgesehen natürlich denjenigen an der Hirnschall"). Obalinski,[6] however, had been meanwhile using Gigli's saw for the formation of osteo-plastic skull flaps, and published an account of the method he employed only a month after Gigli's statement just quoted had appeared. His experience with the instrument had been very satisfactory, and ever since then it has been steadily increasing in favour for this operation. In fact, it seems probable that the instrument will owe a rapid increase in popularity to its value for cerebral surgery rather than to its many advantages in the other departments of surgical practice for which it was originally advocated.

One of the difficulties, one might say the only difficulty, in the use of the Gigli wire saw for the skull, is the separation of the dura mater from the bone. Several instruments have been suggested for the purpose, all of which are probably more or less satisfactory, but none of which have as yet taken the field as undoubtedly the best. Thus Obalinski has used a small elevator; Lauenstein,[7] a piece of watch spring with a rounded end in which is an eye for carrying through a thread of silk; afterwards[8] he recommended a small roller at the end of the watch spring; Gigli[9] a whale-bone sound, working in a director which gives it a horizontal direction—for he took the matter up after Obalinski had drawn attention to it; Braatz[2] recommends loops of wire with different curvatures corresponding to given distances between the apertures, and Gross[10] a special form of grooved director which can be bent as required, and which carries the saw concealed in the groove. Until the saw has sunk into the bone it seems advisable to protect the dura mater when the intra-cranial tension is considerably increased. Some say, however, that there is not much risk of injuring the dura mater if the saw be moved slowly at first, but this probably is only when the intra-cranial tension is nearly normal. However, it would be wise to have some way of protecting the dura mater in case unusual tension were present, and probably a selection from some of the instruments just mentioned will be found best. They have all been employed both for separating and for protecting the dura mater. Braatz's curved wire loops, for instance, might be used for separating the dura mater, working if necessary from each of the two apertures towards the other, and Lauenstein's watch spring might carry through the silk and keep the dura mater out of harm's way.

Having now considered some of the ways of cutting the bone where it has been exposed by division of the soft parts, we must return to the base of the bone flap which has to remain covered up. Wagner advised that this part should be incised with the chisel at one or both sides, and that it should then be broken across by forcibly wrenching up the piece of bone after it had been cut through at the other parts of its circumference; some have wrenched up the bone without any preliminary cuts, but Cotterill has found in the dead subject that this is apt to split the skull towards the base. He accordingly makes a partial saw cut across the base by passing a narrow-bladed saw between the pericranium and the bone. This makes it comparatively easy to crack across the base by forcing up the bone flap. On the other hand, when Gigli's saw is used, as the work is done from within in any case, there is no difficulty in partially dividing the base of the bone flap, and hence no additional instrument is required for this part of the operation. We have only to mention further that Gigli's wire saw leaves as narrow a track as that made with the finest circular saw, and that it is easy to bevel with it by pulling obliquely on the saw as it is worked through the skull. Thus this saw seems to meet all the requirements for the operation we have been discussing, including portability and a moderate price.

Before concluding this general sketch of the osteoplastic operation on the skull, it may be well to state that it does not seem likely to supersede trephining except in certain cases. Professor Krönlein [11] in a recent article, considers that the permanent defect left in the skull by the application of the trephine is not injurious for apertures of 2·5 or 3 cm. (1 inch or 1⅛ inch) in diameter, a size which easily admits of the introduction of the finger. When this size of opening will serve the purpose, and when the position of the necessary opening can be determined beforehand, the trephine should be used. The cases in which he considers it applicable are those for middle meningeal hæmorrhage (although the exact seat of the lesion must be doubtful), drainage of the ventricles, and limited affections of the bone, such as depressed fractures, recent or old standing. On the other hand, where the exact seat of the intracranial mischief is obscure or where it is necessary to gain access to an area of considerable size, he thinks the osteo-plastic method is indicated. The cases where this operation is likely to be called for are those of abscess or tumour of the brain, or where exposure of the cortex is undertaken for its possible removable in epilepsy. We might add to these indications others for simple perforations of the skull of a size even smaller than those produced by the trephine. Such openings being made easily and rapidly might

be used for the introduction of an exploring needle inside the cranial cavity to ascertain if blood or pus were present, for tapping the ventricles (instead of using a trephine), or for injecting tetanus antitoxine into the brain if further experience should encourage us to continue this mode of treatment.

The space at our disposal does not permit of more than a general account of the method of carrying out Wagner's osteoplastic operation, but enough has probably been said to emphasise its importance, and to show that Gigli's wire saw and Braatz's drill are likely to be of much service in bringing it into general use.

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OBSTETRICS AND GYNECOLOGY

BY J. A. C. KYNOCH, M.B., F.R.C.P.Ed.,

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The Uterine Incision in Cæsarean Section.—The credit of perfecting the operation of Cæsarean section is due to Sänger of Leipzig, when in 1882 he introduced the improved technique. By strict asepsis, by complete closure of the uterine wound with multiple sutures, and by accurately judging the proper time for operating, he has almost eliminated hæmorrhage, septicæmia, and exhaustion, formerly the chief causes of the high mortality. In this country the successful results obtained by Murdoch Cameron and others show the advantages of those improvements.

During the past three years the attention of German workers has been largely taken up with considering what is the best uterine incision in this operation. From observations made at a *post-mortem* examination on a full-time pregnant woman, Fritsch of Bonn was struck by the ease with which the pathologist extracted the child through a transverse fundal incision. Fritsch resolved to adopt it in his next case of Cæsarean section. As the result of employing this incision in six cases, he points out the following advantages (*Centrb. f. Gyn.* 1897, No. 20). *First*, the abdominal incision is made higher than usual, so that the umbilicus corresponds to the middle of the incision. This, according to Fritsch, prevents to a great extent,

subsequent hernia, which occurs most frequently when the incision is prolonged downwards. *Secondly*, the uterus being pulled well forward, the transverse fundal incision prevents completely the escape of any fluid into the peritoneal cavity. *Thirdly*, there is practically no hæmorrhage. *Fourthly*, as the lower extremities in the majority of cases present at the fundus, the extraction of the child is greatly simplified. *Fifthly*, the extreme diminution of the wound after uterine retraction makes it necessary to insert only a few deep sutures, and also allows the satisfactory insertion of sero-serous sutures. Fritsch further points out that as the fundal incision is made parallel with the course of the blood-vessels in the uterine wall, and the sutures being inserted at right angles to the incision, the chances of hæmorrhage are greatly reduced as compared with the longitudinal incision, where a divided sinus may lie between two sutures. The proposal of using the fundal incision coming from so eminent an authority, has caused it to be adopted in various continental cliniques, as shown by the publication of sixty-two cases in less than three years. Caruso (*Arch. di Ost. e Gin.*, April 1898) reports four cases where the fœtus was extracted through a sagittal fundal incision, commencing as a buttonhole opening in the centre of the fundus and extending for an equal distance on the anterior and posterior wall. Two of the cases died from causes unconnected with the direction of the uterine incision. Caruso claims priority over Fritsch in suggesting a fundal incision. Müller of Bern, however, had previously recommended a sagittal fundal incision, in which he was supported by Zweifel. The latter, after a short time, abandoned it. Müller (*Centrb. für Gyn.*, No. 9, 1898) recommends a sagittal incision in order to avoid the lower uterine segment. By this method the sutures may be more firmly applied, and, consequently, there is less danger of subsequent rupture of the uterus. He has employed, with success, this incision in the Bern Klinik during the past six years. Braun of Vienna (*Wien. Obstet. Gesch.*, June 1897) was the first to adopt Fritsch's method. By making the high abdominal incision, he was not able to incise the fundus extraperitoneally, and he does not consider Fritsch's proposal a safeguard against hernia. Further, the bleeding was considerable, and he did not find the extraction of the fœtus easier than by the usual vertical incision. As uterine contraction was not, however, specially active, he admits that the hæmorrhage may have been due to primary weak pains (*wehenschwäche*), and independent of the direction of the incision. Knauer (*Centrb. für Gyn.*, No. 3, 1898) reports a successful case of extraction through a transverse fundal incision. Braun-Fernwald, in discussing the case, points out

that as the fundus is frequently thin-walled the sutures may cut through, and, further, that it has not been proved that the cicatrix is as firm as after the usual longitudinal incision. The latter objection has now been put aside in the report of a case by Ludwig, (*Centrb. für Gyn.*, No. 27, 1899) where Cæsarean section had been successfully repeated, previously treated by a fundal incision, and the cicatrix of which was perfectly firm. Braun agrees with Johannowsky (*Monats. f. Geb. u. Gyn.*, Bd. vii., H. 4) that, if a stitch abscess forms in connection with the usual vertical incision, adhesions are set up, favouring the external discharge of the pus, whereas, with the fundal incision, intestinal and omental adhesions are more apt to occur, with a greater risk of general infection. Schröder (*Monats. f. Geb. u. Gyn.*, Bd. vii., H. 2), from an experience of four cases, does not fear adhesions to the abdominal organs, as the uterus, after delivery, becomes anteverted, and the fundus directed towards the anterior abdominal wall. Seidentopf, Kirschoff, and Winckler (*Centrb. f. Gyn.*, No. 4, 1898) report favourable cases; the latter believing that there is less danger of rupture at a subsequent pregnancy when the fundal incision has been made, as this part of the uterus is exposed to less pressure; and he even recommends it in Porro's operation on account of the greater facility of extracting the fœtus. Everke (*Monats. f. Geb. u. Gyn.*, Bd. v., H. 1) reports a case of fundal incision fatal upon the ninth day, from septic peritonitis. This he attributes to gangrene of the edges of the wound, and he believes that ligaturing the uterine blood-vessels at right angles to their course favours necrosis of the wound. Winckler, however, points out that as previous attempts to induce labour had been made in Everke's case, the cause of the peritonitis might be thus explained. Clementz (*Centb. f. Gyn.*, No. 10, 1898), in recording a successful case, puts aside Braun's objection that the fundal incision may not be large enough by quoting Kirschoff's case where a child, weighing 8 lbs., was easily extracted. Steinthall (*Centrb. f. Gyn.*, 14, 1899), in operating on a case of pelvic obstruction from a large subserous fibroid, was unable to extract the child through a fundal incision, and he was forced to combine this with a vertical one. Johannowsky (*Monats. f. Geb. u. Gyn.*, Bd. vi., H. 4), in performing Cæsarean section for the second time upon the same patient, used a posterior vertical uterine incision, and he believes that it may be of use in such cases as Steinthall's. Cohnheim's proposal to open the uterus by a posterior sagittal incision and to leave the wound to drain through the vagina, is not likely to be adopted. Frank (*Gesells. f. Geb. Köln*, June 1897), regarding hæmorrhage as the chief danger to be avoided in this operation, does not con-

sider its arrest to be, in the slightest degree, influenced by the direction of the uterine incision, but depends entirely on the avoidance of the placenta. From an experience of thirteen cases (two fundal incisions with serious hæmorrhage) he prefers a low transverse uterine incision, as recommended by Kehrer, the normal anteversion of the uterus favouring firm union. Riedinger (*Centrb. f. Gyn.*, No. 29, 1898), in publishing two cases, points out that in one the uterus remained permanently fixed high up to the abdominal wall. Perlis (*Centrb. f. Gyn.*, No. 19, 1899), finding the hæmorrhage very slight with the fundal incision, holds that this may be partly accounted for from the fundus being the least vascular part of the uterus (Nagel). Hähn (*Centrb. f. Gyn.*, No. 48, 1899) publishes from Küstner's Klinik eleven cases with two deaths from circumscribed peritonitis. In one there was marked atony of the uterus, and Küstner further believes that the wound may have become infected from contact with the skin of the abdominal wall. He consequently objects to extraction of the uterus previous to making the fundal incision, which may be carried out if a high abdominal incision be made (*Münch. Med. Wochs.*, No. 40, 1899). Although Hübel (*Monats. f. Geb. u. Gyn.*, No. 9, H. 4) states that there is no certainty of avoiding the placenta by making a transverse fundal incision, yet the statistics of Schröder (*Lehrb. d. Geburtsh.*), Güsserow, and Bidder on the position of the placenta, show that a fundal insertion is rare. Hähn, in his eleven cases, came in contact with the margin of the placenta only three times. From the favourable results also published by Reyinga, Jewitt, Cryzewicz, and Walla, and from the continuing satisfactory results obtained in the Bonn Maternity Hospital, it must be admitted that Professor Fritsch, in adopting a transverse fundal incision in Cæsarean section, has added one more to the many improvements he has already introduced in operative obstetrics.

An Operation for Prolapse of the Uterus (*Centrb. f. Gyn.*, No. 2, 1900).—Fritsch refers to the successful results obtained by Freund in cases of extensive vesico-vaginal fistulæ. In this operation Freund opens into the posterior or anterior fornix; draws down the fundus of the uterus; raws the anterior surface, which he stitches to the edges of the fistula, thereby closing the latter. Fritsch now proposes to adopt the same method in cases of prolapse. He regards total extirpation of the uterus and vagina as an unreliable method, on account of the very extensive plastic vaginal operation necessary to guarantee a permanent cure. He seldom performs total extirpation except in cases complicated with fibroids or cancer. Fritsch contrasts

his proposed method with Neugebauers and Lefort's, where the anterior and posterior vaginal walls are directly sutured the one to the other, the results of which, judging from the lack of published successes, are probably unsatisfactory. The following are the steps of Fritsch's operation for cases of complete prolapse:—1st, An oval flap is dissected from the anterior prolapsed vaginal wall; 2nd, anterior colpotomy; 3rd, the fundus is drawn through the opening, and a raw surface dissected from its anterior (previously posterior) wall and sutured to the corresponding vaginal surface; 4th, two flaps are then dissected from the posterior vaginal wall, which are united to a corresponding surface on the posterior (formerly anterior) uterine wall. The operation differs from Neugebauer's in the suspension of the uterus being not direct, but indirect, because the uterus intervenes between the united anterior and posterior vaginal wall. The operation is only used for cases of prolapse after the menopause. Fritsch, so far, has been satisfied with the results. Diagrams describing the operation accompany his paper.

Fatal Case of Non-Puerperal Osteomalacia.—A. Nazari (*Archivio italiano di ginecologia*, Ann. 2, No. 2, April 30, 1899) reports a peculiar case of osseous fragility in a nulliparous woman. The patient, who was nineteen years of age, was brought into hospital suffering from a fracture of the right femur in its middle third. Her mother and one sister had been affected with articular rheumatism with effusion into the knee joints. The patient had been born at the full term, and had been nursed by her mother for three months; she had suffered during childhood from whooping-cough, scarlet fever, and parotitis. She was unmarried and a virgin. She began to menstruate at the age of fourteen, but after the first time the menses were absent for three months, and thereafter were regular but scanty in amount. About the age of fifteen she began to suffer from anorexia, pains in the lower limbs, etc., which were ascribed to anæmia and treated with iron preparations. Her work was long and entailed much sitting. It was stated that the fracture of the right thigh had been caused by a fall on the left side. It had the ordinary characters of a linear, slightly oblique fracture of the middle third of the femur, and was treated in the usual way. Twenty days after admission to hospital the patient, when moving in bed, felt herself unable to move the opposite leg, and examination revealed a fracture of the middle third of the left femur. This second fracture was treated like the first, but neither mended well. A great deal of soft callus was thrown out round the break, but there was no tendency towards con-

solidation, although massage, faradisation and electro-puncture were all tried; and iron, arsenic, iodide of potassium, phosphates and cod-liver oil were given internally. About five months later the patient again had the sensation when moving in bed that a bone had broken; this time it was the left tibia in its middle third. At intervals of a few days other fractures took place, including two breaks in the right tibia, and some weeks later the patient was removed to her own home. For a year she improved somewhat, but then again fractures began to occur, on this occasion in the upper limbs. Finally softening of the ribs commenced and she died, the illness having lasted in all about three years and having included eight fractures. There were no changes in the morphological elements of the blood during this long period, neither did the urine show any chemical or microscopical abnormal elements. A *post-mortem* was not permitted. The diagnosis lay between osteomalacia and idiopathic osteopsathyrosis, but Nazari favours the former. It should be remarked also that phosphorus was freely given without producing any beneficial results.

DERMATOLOGY

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THE army of skin diseases is always enlarging. Among the most recent recruits is the disease described as **Blastomycetic Dermatitis.**

The credit of priority in discovery must be accorded to Caspar Gilchrist of Johns Hopkins, although Busse made his observations independently very soon afterwards. Gilchrist's case is published in full, with numerous microphotographs, in the *Johns Hopkins Hospital Reports.* Since his case several others have been noted, mainly in America, by Hyde, Gilchrist and Stokes, Welles, Hessler, Murphy, and Coates, the most recent, by Anthony of Chicago, published in the January number of the *Journal of Cutaneous and Genito-Urinary Diseases.* To reverse the usual order of procedure, there have been found, invariably in sections and more rarely in the discharged pus, structures which to all appearance are yeasts or budding fungi (blastomycetes). These range from 10-30 μ in diameter, and in Gilchrist's microphotographs are seen quite clearly budding. They were found in considerable numbers in some instances in the corium, thus disposing of the argument that they might be some of the numerous forms of epithelial degeneration. Giant cells were also found there in considerable numbers. In Busse's case,

which was some months under observation, successful cultures were made on various media. These on being injected into animals (dogs and rabbits) multiplied and produced abscesses, which after a time emptied and dried up. One rabbit, inoculated in the peritoneal cavity, died in three days. No results followed the intravenous injection of broth cultures in dogs and rabbits, but white mice died. The organism caused fermentation of grape sugar, with the production of alcohol. Hyde's case was examined histologically and bacteriologically by Drs Hektoen and Davies, whose results were even more convincing than those already noted. Histologically there was that amount of epithelial proliferation which is common to all chronic inflammation of the skin and which is so often regarded by the young enthusiast as proof of malignancy. There were also numerous collections of leucocytes (small abscesses), and in these the blastomycetes were found in greatest abundance. The organism grew readily on all media, best on beer wort, glucose and glycerine agar. The colonies were greyish or yellowish white; on plain agar-agar, a yellowish-brown colour developed. Gelatine is not liquefied, and these authors did not detect fermentation. Their inoculation experiments were even more successful than Busse's, the animals occasionally dying emaciated, with nodules in the lungs and liver in which the organism could be demonstrated. Intra-peritoneal injection lead to the development of disseminated nodules, and when large quantities were injected intravenously the animals sometimes died in twenty-four hours, numerous organisms being found *post-mortem*. There has thus been found in certain cases of chronic inflammation of the skin a yeast-like fungus which can be isolated and which has undoubted pathogenic properties. The nearest approach to the production of a skin disease has been the development of abscesses, but this hardly tells seriously in the argument, for we know how difficult it is to produce lupus by inoculation with the tubercle bacillus. The cases in which the organism has been found have usually been previously regarded as cases either of tertiary syphilis or of chronic tuberculosis of the skin, and their study does lead one to ponder on some old experiences where one was not *quite* sure of the diagnosis, and where one or other was diagnosed because "it must be one or other." Most of the cases have occurred on the hand or leg (Busse's case was exceptionally severe, and terminated fatally), and all seem to have shown that tendency to vigorous growth described as "fungating" (the word "vegetating," which is sometimes used, is an incorrect translation of a German word); in Anthony's case this was so great that the masses had to be drawn aside to enable the edge of the ulcer to

be seen. Facilities for the examination of some of the cases were due to the fact that they were treated by excision, the diagnosis not having been made, but the course of some opens up an exceedingly interesting question of therapeutics. The results of the administration of iodide of potassium have been so happy, that Anthony, in spite of his discovery of the parasite and in spite of the fact that the patient was only forty-four, had ten children, all healthy, and that his wife had never miscarried, cannot "find salvation," and still believes that there was after all a syphilitic element in the case. Hyde (*Brit. Jour. of Dermatology*, July 1899), writing some months previously, answers his arguments completely, and takes the right position that the success or non-success of the administration of iodide by no means establishes the presence or absence of syphilis. The drug must be given in large doses, as much as 80 grains, thrice daily, and in Hyde's case, which, when in hospital, was under the care of Professor Bevan, the stoppage of the iodide was immediately followed by a recrudescence of the disease. Anthony combined the administration of iodide with the inunction of mercury, and thus confused his results. Bevan gave 100 grs. "daily" (?) and applied a harmless dusting powder locally. It is evidently necessary to persevere for some time with the remedy, otherwise relapse is the rule. Our American colleagues are to be congratulated on the definite recognition of a new disease and on the discovery of its antidote.

Another disease on which investigation is throwing fresh light is **Actinomycosis**, and it seems established that that clinical term embraces more than one pathological condition.

Foulerton (*Brit. Jour. of Dermatology*, November 1899), under the title "Streptothrix Infections," discusses the subject and reports a case.

The patient, an Italian woman, æt. 46, was in Middlesex Hospital under Mr Pearce Gould. Twelve months previously an abscess in the left axilla had been opened, and a sinus 5 inches long still remained. For years the patient had suffered from a cough. Then a small red, painful lump appeared above the clavicle, the skin over which broke down about a fortnight afterwards. The skin was deep red or purple in colour, dotted here and there with yellow points—small abscesses containing thick yellow pus. This is excellently illustrated in the chromolithograph which accompanies the paper, in which one further notes the absence of those prominent granulations or "blobs" which are so marked a feature in most cases of actinomycosis. Treatment had no effect, and the patient left the hospital worse.

Examination of the pus and of the granules which it contained disclosed tufts of branching mycelium indistinguishable from those of the ray fungus, but on cultivation another streptothrix growth alone occurred. The growth was feeble, and sub-cultures were difficult to obtain. Two rabbits were inoculated with negative results. Foulerton gives a brief resumé of the cases in which streptothrix varieties have been found. Seven have been observed:—(1) S. Actinomycotica (the Ray fungus); (2) S. Maduræ (the parasite of Madura foot); (3) S. Asteroides, isolated by Eppinger from an abscess in the neck; (4) S. Foesteri, found several times in concretions in the lachrymal canal; (5) S. Odorifera, isolated by Rullman from the sputum in a case showing signs of a localised lesion at the root of the right lung; (6) S. Pseudo-tuberculosa, Flexner, found in a case of extensive pulmonary disease; (7) S. Aurea, found by Dubois Saint-Sévrin in a case of conjunctivitis.

Perthes (*Arch. f. klin. chir.*, lix. 1) examined histologically two cases of **Noma**, and concludes that that disease is a mycosis, due to a parasite occupying an intermediate position between the schizo- and the hypho-mycetes, which he would place among the streptothrichiæ. According to this author noma is not a very distant relative to actinomycosis.

In many cases of actinomycosis the effects of iodide have been distinctly for good, though it has not the remarkable curative effect which it apparently has in blastomycetic dermatitis.

Rammstedt (*Munch. Med. Woch.*, 1899-1900) reports a case of **Malignant Pustule** of the tongue. Patient was admitted to hospital on the third day of the disease. The temperature was 102·5°, the face cyanotic and swollen, as were all the neighbouring parts, and the mouth could only be opened with difficulty. On the under side of the tongue, near the tip, was a well-marked necrotic area, the size of a shilling, covered with slimy secretion, in which the bacilli were found. None were present in the blood.

The treatment consisted in the application and administration of ice, combined with liberal diet and free stimulation with alcohol. On the seventh day the slough was thrown off, and the patient left the hospital in three weeks. The author believes firmly in conservative treatment, and regards any surgical interference as positively dangerous, an attitude in which he has many sympathisers.

Stepp of Nurnberg (*Monatsheft. f. pract. Dermat.*, xi. 29), read before the Medical Society there a note on the use of **Fluroform in Tuberculosis**. Fluroform ($CHFl_3$) is itself

gaseous, but is soluble in water 2·8 per cent. The dose is from one to three drachms four or five times daily. It has little smell or taste, and in superficial tuberculosis its effects are remarkable. A case of lupus of twenty-eight years' duration was selected. The patient took three ounces of the aqua fluroformi daily, the lupus nodules became red, swollen, and painful, were thrown off, and a sound scar resulted. It would not be wise to conclude from this that we have found *the* remedy for lupus, but the remedy is not poisonous, does not interfere with digestion, and seems worth a trial.

Thiosinamin, which was introduced by Hans v. Hebra as a remedy for lupus, has not "caught on." It is a painful remedy, and the results did not seem to justify its employment. Unna (*Monatsheft. f. pract. Demat.*, xxix. 12), states that while he has not seen much benefit from it in lupus, that it is more effectual than any other remedy in **Keloid**. It may be admitted that this statement is not in itself very encouraging, for the remedies for keloid are singularly few and inefficacious.

According to Unna, however, we have in thiosinamin, used in the form of soap or plaster, a remedy of real value in all cases of connective tissue thickening. Keloids, hypertrophic scars, fibrous thickenings in varix, and even small-pox scars, are all improved by the use of thiosinamin plaster. The soap is efficacious, but much slower in action. In the case of scars on the face the plaster may be worn at night only, while the lather of the soap is rubbed in during the day.

Pruritus is but a vague term and articles dealing with it usually merit the same adjective. Besnier (*Jour. des Practiciens*, 1899, 23) divides cases into primary and secondary. If the condition is secondary the treatment of the essential disease is indicated. If the Pruritus is idiopathic (?) strict attention is to be paid to the diet. He forbids game, ragouts, sausages, fat, fish, preserves, cheese, cabbage, tomatoes, spices, alcohol in every form, tea, coffee, and tobacco, and allows fresh roast or boiled beef, green vegetables, cooked ripe fruit, spring water and alkaline mineral water. If the case is very obstinate he restricts the patient to a pure milk diet. As medicine he gives valerian, carbolic acid, musk or Aq. laurocerasi. Spinal douches are sometimes useful, as are a variety of lotions and ointments. Vinegar, one part, water, two parts; carbolic, one, glycerine twenty-five, water, seventy-five; menthol, carbolic acid and camphor, one per cent. in vaseline. If all these fail he uses the constant current. Parisot (*Presse Med.*, 6th May 1899) recommends strict attention to intestinal antisepsis. He orders milk diet, laxatives and benzo-naphthol, two grammes per diem. Du

Castel (*Jour. des Pract.*, No. 19, 1899) has found lactic acid given at the commencement of meals of value in some cases. For children the dose is from six to twenty drops; for adults up to thirty drops daily.

It is pretty clear that after all we may divide cases of pruritus into those of which we know the cause and those of which we do not, and if it sounds more scientific to call these last idiopathic, by all means let us do so. The importance attributed by Parisot to internal antisepsis is probably deserved. The cases of itching of which we do know the causes are often due to the circulation of some morbid product in the blood, and in the absence of other indication, absorption from the intestine is a likely source of some poisonous irritant.

Scabies.—This, the commonest of all skin diseases, is so familiar and the treatment so stereotyped that one rarely sees it referred to in current literature. Sherwell (*Jour. of Cutaneous and Genito-Urinary Diseases*, Nov. 1899), writing, he says, "with a certain degree of impatience," tells us of a method which he has long employed with success and which certainly has the merit of simplicity. The patient has a bath in which he is well scrubbed with sand soap, and a little sulphur lotion is lightly applied. The bed and body linen are changed, and between the sheets half a teaspoonful of sulphur is spread in a train. One of the sheets is lifted and a slight blow causes the dispersion of the sulphur over the whole surface. This is repeated every other night, and in a week the patient is well. Sherwell explains the success of this treatment by the fact that the male acarus does not burrow, that he is either killed or rendered functionless by the sulphur vapour, and that consequently the race dies out. He has never known the remedy to fail and has never seen it cause dermatitis, and he strongly recommends it as a prophylactic measure where crowds are gathered together, as in emigrant ships, camps, etc. Dr Sherwell warns us that this is our last chance; he has published the method twice already and will not do so again.

Notes on Foods, Drugs, &c.

PURE DRY OLD COGNAC

Though whisky has taken its place as a beverage, brandy remains our most reliable stimulant in sickness.

Messrs Ed. Archer & Co. have submitted to us samples of their brandy, which they claim to be of especial value for medicinal purposes. While of high alcoholic strength, it is characterised by a mellow flavour and an entire absence of that sickliness so often found in brandy. The price is, for a really good brandy, very reasonable, and it may be recommended with confidence.

MEDICATED SOAPS, &c.

Messrs Charles Midgley, Ltd., forward us samples of their various products. Scientifically medicated soaps are more familiar to the majority of medical men as German products, in which country they were first perfected, and where a co-operation between physician and manufacturer is differently regarded than in this country. Midgley's soaps have largely the same composition as Eichhoff's, and being familiar with both we can only say that they are at least as well made as the originals, and that their price is reasonable. They supply cake soaps, both alkaline and superfatted, variously medicated, and they have also a long list of "powdered" soaps, which have the advantage that only as much as is required is used, and the chances of decomposition are reduced to a minimum. We would especially commend their "Silicon" soap, an alkaline base containing 10 per cent. of purified natural silicates, as an unirritating soap for habitual use.

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We have received from **Mr George Lunan**, Pharmaceutical Chemist, 20 Queensferry Street, Edinburgh, samples of his Compound Cachets and Capsules of the Glycerophosphates. They are elegant in appearance and afford probably the best mode of administering the glycerophosphates from the fact that in solution the salts are unstable. In a cachet or capsule, however, the salts of glycerophosphoric acid are absolutely stable. The tonic and nourishing effects of the glycerophosphates are held by many to exceed those of the hypophosphites, and these cachets or capsules afford a trustworthy method of exhibiting them.

Mr Lunan shows the exact formula to any member of the medical profession making direct inquiry.

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THE SCOTTISH
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JOURNAL

ON HEREDITY IN DISEASE[1]

By D. J. HAMILTON, M.B., F.R.S.Ed.,
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ALLOW me, in the first place, to thank the Council of the Society for the very great honour they have conferred upon me in asking me to take part in this discussion, and more particularly for the place they have assigned to me in the order of debate. When I think of the many members of this Society who have made heredity a special study, I feel the honour to be all the more flattering.

It will be acknowledged on every hand that the subject you have chosen for debate is full of interest; an interest which has become, if anything, more intense in late years than formerly, from the vast amount of literature, biological and purely medical, which has found its way into our midst, dealing not only with concrete examples, or supposed examples, of acquired characters being hereditarily transmitted, but going to the very root of the matter in speculations as to what heredity depends upon, and of what the mechanism consists which underlies its manifestations. It is one of the commonest experiences of life that not only the general conformation of the body may be transmitted from parents to child, but that the same holds good of the minutest details of conformation. Further, it is matter of common note that the most subtle mental features and traits of character may, similarly, be handed on from one generation to another. We have little

[1] Paper read before the Edinburgh Medico-Chirurgical Society on March 7th, 1900, introductory to a discussion of the subject.

experience of how far the finer manifestations of heredity prevail in unicellular organisms, but in the higher metazoa the phenomena are so blatant as to have attracted attention in all ages.

The subject of discussion this evening is the influence of heredity in disease, and here it will be advisable to recall the fact that mere congenital transmission of disease from either parent to child is not heredity. Most of those diseases which are transmitted to the fœtus *in utero* are of the contagious type, and, consequently, are to be referred back to the action of a microphyte. Such transmission of contagious disease is, of course, to be accounted for by direct inoculation of the fœtus, in most cases through the system of the mother. It used to be supposed that the placenta was a perfect filter against the transmission of contagion of the particulate kind; it was taught, for instance, that anthrax is never conveyed from the blood of the mother to that of the embryo. And although the approach of the placenta to perfection in this respect is undoubted, yet now and again it fails, and the contagion passes over to the offspring. Diseases of the contagious type seem to differ in the facility with which they are transmitted by this means. Thus, in the case of anthrax and tuberculosis, the infection of the fœtus through the mother occurs only very rarely, while we know that in that of syphilis the liability is extreme. These cases, however, of intra-uterine contagion hardly come within the scope of our discussion, unless in so far as they can be utilised for the purpose of proving or refuting the alleged transmission of a disease which has been acquired.

Of all the numerous theories to account for the handing down of hereditary characteristics, there may be said to be two which have met with most acceptance; not that they have much in common, but rather because they represent the two points of view from which the matter is regarded. Indeed, it may be said that nearly all other theories which have gained currency are to be looked upon as modifications or extensions of these. The one, it will be remembered, is known as the "Pangenic Theory" of Darwin, propounded by its author, with his characteristic modesty, as a mere provisional or tentative explanation of how acquired features may be trans-

mitted; the other, that of Weissmann, known as the theory of the "Continuity of the Germ-plasm."

Between what we recognise as living protoplasm and its ultimate molecules, there are probably several stages of subdivision, molecular groupings, to the lowest of which various names have been given in keeping with certain theories of vitality bound up with their supposed existence. Thus these supposititious supra-molecular bodies have been termed pangens (de Vries), gemmules (Darwin), micellæ (Nägeli), idioblasts (Hertwig), plasomes (Wiesner), and biophores (Weissmann). They all refer to what may be regarded as molecular aggregations, which, by their association and interaction, go to make up what is roughly termed in biological language "living protoplasm." According to Darwin, these excessively minute particles of living matter, "gemmules," as he named them, are constantly being given off from the body-cells and circulate freely throughout the system. They were reputed by him to collect in the reproductive-cells, and to imprint upon these the features which the somatic cells had acquired through external agencies. Thus they tended to proceed centripetally, to convey to the reproductive-cells those impressions which had been received from without, and to transmit these to the offspring. The inheritance of acquired characters, according to Darwin's hypothesis, was therefore habitual.

Weissmann, on the other hand, takes the view that germ-cells (ova and spermatozoa) and somatic-cells differ, in so far, that the germ-cells are the exclusive bearers of hereditary qualities, the transmission being effected through the instrumentation of what he calls the "germ-plasm." This "germ-plasm" is comprised in the chromosomes of the ovum and spermatozoön, is continuous from one individual to another, and is, therefore, practically immortal. He would regard the germ-cells as an exclusive royal line, as it were, handed on from generation to generation, and having no community, so far as the propagation of hereditary qualities is concerned, with the cells of the soma or perishable part of the organism. All new hereditary features are the result of variation in the structure of this "germ-plasm," and are brought about by the recurrence of slight inequalities of nutrition. He ignores the influence of external agencies acting upon the somatic-cells as

an element in the production of heredity; he believes that only those variations which are blastogenetic become hereditary, those which are somatogenetic are not so. His theory supposes that the particulate elements of heredity proceed exclusively in a centrifugal direction, and is therefore opposed to that of Darwin. The inheritance of acquired characters, according to his theory, is impossible.

It will be evident that the whole matter of the alleged transmission of disease hereditarily hinges upon this debated point, as to whether acquired characteristics can be inherited; in other words, whether external agencies can so impress the soma, and through it the "germ-plasm," that the effects of these impressions become hereditary. Hæckel, Virchow, and Eimer are at one with Darwin in the belief that they can, and the evidence they have brought forward in support of the assertion is voluminous, in many cases striking, but by no means in all instances convincing. Virchow is a strong upholder of the theory of *causæ externæ* generating effects which are inherited, and it matters not whether these act on germ-cell or body-cell. "A living being placed under varying circumstances changes its functions and habits, and what it acquires it can transmit hereditarily." On the other hand, we have Weissmann, Ziegler, and others, just as strongly convinced that acquired properties are not inherited, but that hereditary tendencies are due to variation in the structure of the "germ-plasm."

The supposed transmission of *mutilations* I shall pass over in a single word by asserting that there is no evidence in support of the view. The Jewish and other Eastern nations have been circumcising since the days of Abraham, and probably long before that period, and I have yet to learn that Jewish children are born without foreskins.

Can we say, however, that an external agent of any kind giving rise to a morbid state of the body, ever so impresses the cells of the body that the disease becomes established as a hereditary peculiarity? Let me take up some of the best known examples of so-called hereditary diseases with the view of discussing them, and of calling forth the valuable experience which members of this Society must be able to bring to bear upon the subject.

Perhaps no disease is held to have so hereditary a tendency, at least in the layman's mind, as tuberculosis; and it is undeniable that the disease affects different members of the same family, and that the predisposition to it can be traced back in the ascendants as a distinct family strain. If I had come before this Society previous to the discovery of the tubercle bacillus, and had asserted that the disease is not hereditary and very seldom congenital, my remarks would have met with little credence. Yet such is the general conclusion of pathologists at the present day. With extremely few exceptions—so few that they may almost be neglected—children are not born tubercular even of tubercular mothers, nor are the young of the lower animals born tubercular under like conditions. Even in these exceptional cases there is the possibility that the mother suffered from genital tuberculosis while pregnant, the transmission of the disease under such circumstances to the developing embryo being, one would say, a very likely accident. The fact, however, that such contamination of the embryo within the tissues of the mother happens so seldom, shows with what difficulty the transference is effected. It has been asserted, but proof is certainly wanting in confirmation of the allegation, that the ovum may become infected through the spermatic fluid emanating from a person suffering from tuberculosis of the testicle or its adnexa. That the ovum at the time of impregnation may become inoculated, that in a manner it may become fertilised by the spermatozoid and inoculated with the bacillus of tubercle simultaneously, I cannot imagine to be true. After conception has occurred, and the embryo is fairly advanced in development, there is a possibility of such an occurrence, but to my mind the likelihood of its taking place even then is extremely small.

Putting, therefore, the view of tuberculosis being congenital out of court, we have to fall back upon the explanation of the undoubted proclivity which the disease has to run in certain families as dependent upon one of two factors—either direct inoculation of the individual in extra-uterine life, or the inheritance of a particular predisposition, the tubercular temperament. No doubt when a member of a household becomes tubercular, the tendency to infection of other members, through family relationships, is very great, but this cannot be held to be the

only cause of the disease selecting such families, and, it may be, affecting several generations in succession. Every physician and surgeon knows that the members of such families are, in a large number of instances, notable as tubercular subjects from their conformation of body and the liability they manifest to certain diseased conditions, not necessarily tubercular, but which nevertheless are sufficiently striking to confirm the suspicion of the hereditary taint. I need not enter into details of what this habit of body is characterised by; the features I am referring to are matters of daily experience with you all. My argument is that it is this habit of body, not the disease itself, which is inherited; and if you asked me in what the particular vulnerability consists, I should reply that, most likely, it resides in the epithelial protective coverings of the body being too little resistant, too easily stimulated by external agencies, too readily penetrated by the parasite of the disease. There is good reason to believe that, instead of Man being an animal very prone to tuberculosis, he is extremely insusceptible, otherwise it is difficult to explain how tuberculosis has not utterly decimated the human race. When tuberculosis is freshly introduced among savage communities, such decimation has been known to occur. Civilised nations have probably become in a manner hardened against the ravages of the parasite. There is no reason to believe, however, that any member of the human race is immune to its influence, in the sense that certain animals are immune to parasitical diseases which are peculiar to Man, or in the sense in which an animal may be rendered immune artificially. There is good reason for affirming, on the contrary, that there are few members of the human race who could not be inoculated with the tubercle bacillus were it implanted in their tissues.

The lung is the portal at which the organism usually gains admission, and here, it will be remarked, the protection of the surface consists simply of a delicate film of epithelial cells. In children and young persons with the tubercular habit of body, this epithelium tends constantly to be thrown into a state of germination, rendered apparent by slight attacks of bronchitis and catarrhal pneumonia, the desquamation accompanying these conditions exposing the underlying parts and thus encouraging the bacillus to take hold upon them. We have

analogies bearing upon this in the ease with which the phylloxera penetrates the epidermis of the French vine, and in the difficulty it has in making its way through that of the American vine, which is thicker and more resistant. The organisms of putrefaction, and probably of septic disease, may be introduced into the healthy bladder of an animal, or that of Man, almost with impunity; while, if introduced into one whose epithelium is in a state of catarrh, and whose deep parts, consequently, are exposed, the danger of communicating septic disease is extreme. We do not know whether, in the case of individuals with the tubercular habit, the epithelia in other parts of the body, such as that covering the intestine, are to a like degree vulnerable. The fact that so many of us can consume tubercular milk without becoming tubercular, would tend to show that, under normal circumstances, the power of resistance possessed by the alimentary canal against this parasite must be very great.

What I would suggest, therefore, as inherently bound up with the hereditary tendency of tuberculosis, is this vulnerability of the protective epithelia, and this, there is every reason to believe, is handed down from generation to generation. In support of this assertion are to be taken into account certain epithelial manifestations which accompany the tubercular habit—namely, the very dark or very light degree of colour of the hair, the overgrowth of hair in the bushy eyebrows and long eyelashes, and, lastly, the occurrence of a lanugo-like overgrowth in tubercular children along the spine and over the legs. To my mind, these all point to an anomaly of the epithelial type which is peculiar to the tubercular habit of body.

Granted, for the sake of argument, that this habit of body, which is handed on from one generation to another, is the cause of the hereditary disposition to tuberculosis, and that it manifests itself in certain peculiar anomalies of the epithelia, we may ask, in reference to the subject of debate, how it is that this habit of body has arisen? Where has the inherited strain come from? What is its ancestral history? Can it be generated by vicious surroundings? I question whether it can. No doubt, once in the blood, the particular habit may be fostered by every external agent which tends to deteriorate

the natural powers of resistance. But will such external agencies tend to produce a particular colour of hair, a certain narrowness of chest, tallness of stature, and other peculiarities which are distinctive of the tubercular constitution? My conviction is that they will not, and that we must go much further back in the history of the human race to get at the explanation of the matter. My own impression is that these features are the lineal descendants of a variation which took place far back in our history, that the variation has occurred irrespective of surroundings or external agencies, and that its influence has been propagated in the descendants ever since. It may be a variation which is common to many races, but one which apparently is intensely hereditary. When we see how racial peculiarities are propagated for ages, how the type of character no less than the lineaments of a race continue very much the same through all time, is it very Quixotic to suppose that a certain type of constitution has in a like manner become a race inheritance? It seems to me, on the contrary, to take a concrete example, that if the spirit of commercial enterprise exemplified in that memorable transaction between Abraham and the children of Heth, concerning the purchase of the cave of Machpelah, be still the spirit of the Jewish nation, there is no reason to believe that a variation of structure predisposing to disease may not have been handed down to us from remotest times.

The second so-called hereditary disease which I wish to bring before your notice for critical examination is gout. There is no more common belief than that this disease is engendered of high living, and that, once established, it is capable of being hereditarily transmitted. In a certain sense this notion is correct, and in a certain sense it is, to my way of thinking, entirely erroneous. There is no doubt that once the gouty tendency has been made manifest by the external agencies referred to the disease will most likely show itself in the next generation, but not necessarily so; it may skip a generation and reappear in that which follows. But are we quite sure that the gouty habit, and I employ this term in its widest sense, has originated in the abuse of articles of diet? There is such a thing as poor man's gout, and as Mr Jonathan Hutchison very properly remarks, there are certain individuals

in whom no amount of abuse of either food or alcohol will excite gouty manifestations. The gouty individual is one with a peculiar habit of body, showing itself not only by the deposition of uric acid in certain tissues, but by modifying almost every function of the body. This habit is accompanied frequently by high arterial tension, and is followed, in course of time, by degeneration of the kidney and blood-vessels. Is this complex of phenomena traceable to any external agent such as high living? Or is the external agent simply one means of rendering the inherent vice apparent? The presence of uric acid in the blood and tissues has been held by Garrod to be the diagnostic feature of the disease, and this formation of uric acid has by some been looked upon as evidence of sub-oxidation of nitrogenous waste. We know, however, that reptiles and birds excrete a large part of their nitrogen in the shape of uric acid. May we not entertain, therefore, as a possibility, that the gouty constitution, so-called, is in part a reversion to some far back ancestor in which uric acid was excreted normally to a much larger extent than it is at present in an average member of the human race? It has always seemed to me that one great reason why it tends to be deposited in the tissues is the fact that the kidney, owing to its degeneration, fails to excrete it.

The conclusion I have arrived at as regards the origin and heredity of this disease is very much the same as that bearing upon tuberculosis, namely, that the gouty habit of body has arisen as a variation, and as such is hereditarily transmissible, and that excess of diet and alcohol merely render the habit of body apparent.

I come next to the great class of mental diseases, those diseases which manifest themselves in connection with what is termed the neurotic or the psychopathic constitution. In approaching this subject, it is to be borne in mind that habitude, sensation, perception, association of sensations, and images, in one word, the modality of the spirit are bound up indissolubly with organic substrata, and as these are hereditary, it follows that intellectual aptitudes are so also. As Debierre puts it—"Man thinks and acts, not spontaneously, but according to the blood which is in his veins; that is to say, according to his heredity. He thinks, he feels, he wills much

more through his ancestors than through himself." Thought is nothing more than a secretion, the result of a certain metabolism; and it stands to reason that from time to time, in the phylogenetic history of a nervous system so complex as that of Man, variations in the durability and resistance of the mechanism underlying this metabolism must have taken place, as we know to have occurred in the internal mechanism of other organs of the body. Mental derangements, we know, are among the commonest of diseases. It may be that the individual is not so far deranged as to require restraint. There are minor degrees of mental derangement which may be included under the terms wrong-headed, eccentric, fanatical, or hypochondriacal, which may manifest themselves in a line of psychopathic individuals, which are quite distinctive of the type, and which may break out at a certain period, and in a particular member of the family, in one or other of the various definite modes of what is commonly termed mental alienation. All such manifestations indicate a certain diminished resistance or power of endurance on the part of the nerve-cells of the brain; and in this relationship, and in reference to what I have indicated as my own persuasion of the essential element in the heredity of the tubercular constitution, it must be borne in mind that nerve-cells are of epidermic origin, and that the tubercular constitution is frequently associated with the psychopathic. I have attempted to trace the predisposition in tuberculosis to a want of resistance on the part of the epithelia to the encroachments of the tubercle bacillus. May not there be a certain connection between the two classes of disease in respect of their representing, in reality, the same hereditary peculiarity, the same dyscrasia, the same tendency to decay on the part of nerve-cells and epithelial-cells?

I have often thought that another factor predisposing to mental derangement is, that nerve-cells show so little capability, if any at all, of regeneration. We are born apparently with the nerve-cells which will serve us throughout life. Is it a matter of wonder that frequently these break down under unusual strain, or what is probably much more likely, as an inherited peculiarity? The germ-track followed in the ontogeny of the nerve-cells is very short, far shorter than in the case of many other cells throughout the body, and hence a state of maturity

is reached at a comparatively early period, with an inclination to premature decay.

But in this class of diseases, as in others which may be considered in the ordinary acceptation of the term to be hereditary, the great question continues to assert itself, namely, whether external agencies can bring about a state of the nervous system which is distinctly morbid, and which can be transmitted for generations in the offspring. The evidence in support of the positive view of this question which is usually quoted, is that of the production and hereditary transmission of epilepsy in guinea-pigs. Brown-Séquard's experiments appeared to show that after hemisection of the cord or division of the great sympathetic in the neck, not only did the animals become epileptic, that is to say, not only could they be readily thrown into a convulsive fit, but that this peculiarity of constitution, thus engendered, could be transmitted to their young. The operation on the sympathetic brings about a trophic lesion of the eyeball which it was asserted is also transmitted. I must confess it has always seemed to me that an element of fallacy has entered into these experiments which would require to be eradicated before we can found any conclusions upon them. Have we crucial evidence to show that a mental disease may be excited through external agencies, as, for instance, by the abuse of alcohol, in a person free from any ancestral taint, and that this disease so excited can be transmitted through several generations. My own impression is that we have not; but it would be a matter of extreme interest to me, as I daresay to other members of this Society, to hear of any confirmation of the allegation? So far as I am personally informed, I feel that, in mental derangement, and in excess of perhaps any other form of disease, we have to do with an inherited peculiarity or variation, a variation which may have occurred in a far back ancestor and lain dormant for many generations, but which inevitably manifests itself under conditions of unusual external stimulation, and which is in no respect bound up etiologically or necessitated by this stimulus. The substratum which underlies the mental peculiarity is allied to that underlying the predisposition to tuberculosis, or gout, and, probably, is referable to a fault in metabolism excited, it may be by an inherent bias towards degenera-

tion in the nerve cells of the brain, and this is eminently hereditary.

From what I have already said it will be apparent that I do not regard syphilis as a hereditary disease. Congenital it certainly is, but I fail to note that, once acquired, it can be transmitted as a truly hereditary affection. As in the case of most contagious diseases, the contagion can pass from mother or father to child, but has an inevitable tendency to die out in the early years of life in the first descendant. This is not heredity but intra-uterine contagion.

Hæmophilia appears to be, in many instances, a truly heritable disease, and so far as our lights go in pointing its pathology, they seem to indicate that it is more a disease of the blood-vessels than one of the blood. The vessels do not seem to contract as they ought to do, and their coats are peculiarly friable. One could quite well understand this peculiarity being transmitted, just as the tendency to hæmorrhoids is said to be transmissible on account of the veins concerned having unusually thin walls. I have often thought, however, that there is something more in hæmophilia than this mere anatomical peculiarity, and that there may be a close relationship between it and menstruation. The fact that it is transmitted in the male line is significant. It has often been alleged that menstruation occurs in the male as well as the female; that although there is no actual discharge of blood, there exists the monthly constitutional disturbance of the general functions in the male as well as in the female, accompanied, it may be, by rise of temperature, a tendency to undue relaxation of the blood-vessels supplying the organs of generation, and possibly also of the hæmorrhoidal vessels. There seems to me to be a good deal of truth in this allegation. May not hæmophilia be simply a manifestation of this menstruation in the male?

Many more examples might be brought forward illustrative of the thesis I have started with in my remarks, but the above will probably suffice to elicit the opinions of others.

Turning now to another matter, I would ask the attention of the meeting to the consideration of what evidence we have of a hereditary tendency to disease becoming eradicated by inbreeding with individuals having no such tendency. How

soon can we assert that the hereditary psychopathic constitution becomes eradicated in the descendants under such circumstances? Or, does it become eradicated at all? If we accept the statement that the extrusion of the polar cells with their quota of chromosomes before fertilisation occurs represents a true reduction process, that at this time a certain part of the maternal heredity is got rid of by this means, have we any reason for believing that, in course of time, a hereditary vice of constitution may entirely disappear? And here the facts of atavism come in to our aid. Polydactylism in man is supposed to be a reversion to a heptadactylous ancestor; the striping of mules is said to point back to an early equine ancestor, and horses and asses are said to contain solitary "zebra" determinants in their germ plasm; the three-toed horse anomaly is alleged to be a reversion to the fossil hipparion of the eocene period; the lanugo of the fœtus, a reversion to a hair-clad ancestor. I am told by a breeder of black polled Angus cattle that the progeny of a perfectly black polled bull and cow is sometimes a red calf with horns; indeed, unless the pedigree of mother and sire is pure for generations back, that no reliance can be placed upon the progeny they may beget. If these statements be true, what are we to say of the transmission of variations in constitution? My firm conviction is that if a vicious line is introduced it may die out, and probably does in most cases die out by interbreeding with a series of pure stocks, but that no reliance can be placed upon its not recurring atavistically, it may be, generations afterwards.

Lastly, let me refer briefly, and it must be briefly, for I am afraid I have already exceeded the limit of time put at my disposal, to the very interesting subjects of maternal impressions and telegony in disease. The influence of the mother upon the fœtus *in utero* must be very great, perhaps far greater than is generally admitted, in so far at least as nutritional factors are concerned. The subject of maternal impressions, however, is open to many fallacies, mere coincidences being interpreted as evidence bearing upon the matter. For my own part, I have little belief in maternal impressions as a cause of malformation or other peculiarity. As an example of the sort of evidence usually brought forward in support of the theory, I may refer to the case mentioned by Hippocrates of the white

woman who bore to her white husband a black child, and who explained the occurrence by there having hung in her bedroom a picture of a black man—and so on it goes. Have we any proof of the occurrence of telegony in disease? From the evidence collected by Romanes and others, there seems to be no doubt of the mother being impressed by the male element of a first pregnancy, in such fashion, that the subsequent progeny by another male partake of the features of the father of the first born, and I daresay Professor Cossar Ewart may have something interesting to tell us on this point. But what I would endeavour to elicit from the discussion, this evening, in this relationship, is whether we have anything analogous in the transmission of hereditary diseased states of body. Are there any grounds for believing, for instance, that a father with a strong tubercular hereditary history, and bearing the lineaments of such in his person, can so affect the system of a healthy mother that the children subsequently born to the same woman by a second husband, with no such strain in his blood, and no such lineaments, have the tubercular characteristics of the first husband impressed upon them? I know of no evidence bearing on the problem, but possibly some members of the Society here this evening can throw light upon it.

In conclusion, allow me to formulate the substance of my remarks in the following synopsis:—

(1) There is no evidence proving that diseased conditions of body, excited by external agencies, using the term in its broadest sense, can be transmitted hereditarily through generations.

(2) That the various hereditary tendencies or predispositions to disease of the hereditary type have arisen as variations in the germ-plasm.

(3) That these predispositions to disease probably extend far back into the history of the human race, and break out only occasionally, in accordance with the laws of atavism.

(4) That external agencies are merely the means of bringing them to light.

(5) That there is little if any reliance to be placed in the evidence bearing upon the influence of maternal impressions.

(6) That there is no reason to believe that telegony may

not prevail in the case of hereditary predisposition to disease as it evidently does in regard to other characteristics.

Discussion

Dr Clouston, in opening the discussion, said that Prof. Hamilton had introduced a very vast and intricate subject, indeed a series of vast and interesting subjects, which to discuss thoroughly would take at least a winter session of this Society —subjects about which half a dozen treatises could be written. He was sure he expressed the minds of members of the Society that they were all deeply indebted to Prof. Hamilton for the way in which he had brought the matter forward.

With regard to the theoretical view of heredity now most generally accepted among biologists—that of Weissmann—it seemed to him that Weissmann had made three very striking admissions in regard to his general thesis that the effect of acquired characters cannot possibly be transmitted. In the first place, he admits that the germ plasm can be infected by such a disease as syphilis, so that thus the disease may pass from parent to child. His second admission is that the law of non-transmissibility of acquired characters does not apply to the very lowest organisms. The third admission is most important to medical men, who, he assumed, looked upon the matter from a practical rather than a theoretical point of view. It is that environment, especially climatic conditions, has the power of influencing the nutrition of the germ cell itself, and so altering its constitution that you may have an organism produced which is in some respects different from its parents. There was no doubt these admissions went some way to satisfy the universal experience and the universal instinct of the profession as to the heredity of disease.

In regard to the psychopathic constitution, he thought there was no doubt on the part of all who had looked into the subject, that it was well proved to be most hereditary. For two hundred years or more one of the royal families of Europe, whose history is well known, have undoubtedly exhibited this constitution in all its enormous varieties. Confining his remarks to the tendency of hereditariness in the neuroses, it always seemed to him to be an important admission on the part of Weissmann that environment and evil climatic conditions, and among these we must include evil dietetic conditions, may affect the germ plasm. Looking at heredity, particularly from a physiological and pathological point of view, what is the most important thing that good heredity can give the individual?

He would say, in the first place, the power for the individual—the product of the germ cell and the sperm cell—to integrate, to develop and maturate on normal lines, from the moment of the first contact up to the full development of the individual. Assuming that you can have a certain debasement of this power, such an alteration in the nutrition of the germ cell as to affect it unfavourably, had we not in this way a practical explanation of a vast number of the neuroses? Taking the neuroses one by one, it would be found that by far the most important arise during the period of development through some defect in the maturation of the cells and of the tissues. In the case of epilepsy there was no doubt whatever that at least three-fourths of all epilepsy occurs during the period of development, and this was also the fact with regard to St Vitus' Dance and the great crowd of other nervous diseases, irrespective of the great number of congenital defects, idiotcy, imbecility, and teratological defects generally. In regard to insanity also, there was no doubt that mental alienation occurred in its most marked and characteristic forms during the developmental period. Looking at the brain cells in the mental area, if we examined a typical cell by the three or four processes best adapted for showing its constitution, it would be seen (and he thought Professor Schäfer would bear him out in this) that there was no more integrated and complicated structure in the human body than one of these larger brain cells. The Nissl's bodies, the reticular network, the fibrillation, the peculiar structure of the nucleus and even in the nucleolus—all this made one amazed at the enormous complication of this one cell; and there are in the brain something like three or four hundred millions such. Considering the integration of such a cell from the embryo up to the age of twenty-five, and thinking first of its marvellous structure, and secondly of its function, mentalisation in its numerous forms, and the connection of mentalisation and emotion with nutrition and with all the bodily functions, we can imagine how very small a difficiency in the nutrition of the germ plasm—how very small a change may mean some arrest, some debasement, or stoppage of the developmental process in groups of these cells; and such a small change he assumed to be possible on Weissmann's admission. In this he thought we had a theoretical explanation of certain hereditary defects. They were defects in the development of the nerve centres, arrests of maturation of the cells that constitute these centres. In this way we had a basis for the heredity of a large number of the neuroses.

There was no doubt whatever of the non-resistiveness of certain tissues about which Professor Hamilton had spoken.

Take for example ringworm. It occurs in the child simply because its epidermis cannot resist the spores, while that of the grown-up individual can do so. Applying the same principle, he thought we should find that a very slight change indeed in the resistiveness of the brain cell would explain the well-known want of resistiveness to the numerous mental and bodily disturbing influences that act on the developing brain cells and cause adolescent insanity and other neuroses. Looking at the influence of the sexual and reproductive instinct, it was known that at a certain period of development there is an enormous strain on the constitution of every girl and every lad. Assuming that we had a slight alteration or even a germ infection of the germ plasm causing an arrestment of the developmental process; we had, he thought, an explanation in that way of a hereditary tendency. Certain individuals had weakness in the nutrition of the germ cell, and on account of this had not the power to develop into full maturity, this defect recurring in successive generations.

Then taking the other process—that of decay—the normal physiological process of growing old, coming to an end, and dying; we know that in certain families there are many individuals who become insane at the turn of life, as they become old, simply from an unphysiological method of nerve disintegration. From this cause there was a whole set of neuroses of what one might call the decadent period. Assuming that through defective germ plasm the brain cells are deficient in another great power of heredity, viz., that of carrying the individual physiologically through his decadence; there was here a theoretical explanation of the heredity of these diseases of the decadent period.

With Professor Hamilton, he did not believe, nor did he think anyone did, that mere personal mutilations, or the effects of surgical operations could be transmissible. Nor did he believe in Brown-Sequard's experiments, and he thought that Weissmann upsets to a large extent the conclusions that were founded on these experiments.

With regard to the alcohol habit, in which they were all profoundly interested, there was no doubt as to the actual fact, whatever might be the explanation, that in certain families there runs a tendency to the abuse of alcohol. He would so far agree with Dr Reid, writing lately in the medical journals, that no man has a born tendency to take whisky; but he thought a great many men unfortunately were born to what might be called a lack of higher inhibitory mental powers. He held that this was strictly analogous to the lack of developmental power in the man who was affected with adolescent insanity. There

existed no doubt in these individuals a morbid reactiveness to the alcoholic stimulant. A young man with this highly neurotic tendency taking two glasses of whisky may go clean off his head, and he would lose mental power and strength of mind with each repetition of the dose; and would settle down at length into a dypsomaniac of the very worst class, viz., one that begins during the period of development—a class of which he was sure all present must have had experience as a nuisance to themselves and to the world. It was not the craving for alcohol that was inherited, but a general psychopathic constitution in which the alcoholic stimulus is an undue stimulus, and the mental control deficient. He thought it must be admitted that it was essentially a hereditary weakness in the brain reactiveness to alcohol; some people being able to take a large amount with impunity, others not being able to take a drop with any impunity whatever. A neurotic heredity is thus seen to resolve itself into general morbid tendencies rather than direct proclivities to special diseases; and this seems quite compatible with the Weissmann theory of heredity.

He was struck with one of Professor Hamilton's observations —that with reference to the Jews. Now in Jerusalem he once counted four distinct types of Jews—all quite different—so that he thought the Jewish element of permanence was really not so great as has been supposed. He saw the common type of dark complexioned, hook-nosed Jew; the red-haired, turn-up nosed Jew; the Georgian, tall, proud-walking Jew, fully armed with pistols and poniard; and the Indian Jew of the Hindoo type.

DR JOSEPH BELL was not prepared to take part in the discussion, but thought that in the course of a few minutes he might bring out that there was some ground for the belief in heredity from the surgical as well as from a public point of view.

A Board of Insurance would not pass a man who had three aunts and two uncles dead of tuberculosis without a certain amount of loading. If his ancestors have had cancer, they will take a similarly serious view of the case. He thought, however, that tuberculosis, syphilis, and cancer might be put aside as not being hereditarily transmitted diseases, but which might be transmitted in the germ cell to the germ cell as has been mentioned.

There was one little point in surgery which always struck him as interesting, and about which he had no doubt; that was the hereditary tendency in certain families to the formation of those small cystic or encysted tumours called wens on the head

and upper part of the body. On every opportunity he had inquired of the possessor of one of these wens as to their occurrence in his ancestors, and had nearly always found this to be the case. Now the child was not born with one of these tumours, but what was transmitted was the hereditary tendency, some defect in the epithelium or in the sebaceous follicles, which at the age of thirty-five, forty, or forty-five, began to show itself in a large number of members of certain families. In connection with these matters of heredity there were, however, certain fallacies. If a woman thinks she is going to have cancer, she will be pulling about her breasts and brooding over the matter, and will be much more likely to get the disease than if she did not think of the matter at all. He had seen many cases in which a perfectly healthy woman had worried herself into the belief that when she came to the age of forty-five or fifty she would have cancer, because some aunt or other was afflicted at that age—at anyrate, something was produced which had to be removed, and which recurred and gave a lot of trouble.

There was a great tendency towards the inheritance of certain abnormalities of development, especially to hare-lip and cleft palate. But even here there might be a fallacy, as was instanced by the case mentioned by Professor Haughton of the young cubs which all died of hare-lip, but which was at length explained by the fact of the keeper having fed the lioness while in the family way on tit-bits, but without bones; and when the bones were subsequently supplied, the *hereditary* tendency to hare-lip entirely disappeared.

The whole subject was full of fallacies; he had, however, never heard a more interesting paper on the subject than that of Professor Hamilton, nor one which had introduced such a vast amount of discussible matter.

Professor COSSAR EWART would first congratulate Professor Hamilton on having arrived at a very orthodox view of the subject. He thought that on nearly every point Professor Hamilton's opinion was, from a biologist's point of view, as nearly as possible orthodox. On the other hand, he should say that Dr Clouston represented the opinion still held by the vast majority of medical men—a belief in the transmission of acquired characters. In Dr Clouston's remarks, one felt that all through he argued in favour of such transmission. There was, however, no single point that he could take hold of to controvert; he would therefore leave him to be dealt with by other speakers.

With regard to Professor Hamilton's first point—that in the case of gout, tubercle, and other diseases, they referred to a long

time ago variation, which led to the appearance of gout in one family and tubercle in another; and that all the tubercle in the world, or most of it, has been inherited from previous variations. I don't think it is necessary to go so far back for either gout or tubercle. The variation that occurred a million years ago or a thousand years ago might also occur to-day. There was nothing that struck him more than that variation was enormously common. It was the easiest matter to produce a perfect epidemic of variation. Domestic animals reproduce themselves with great uniformity if kept apart; but the moment one mixed up two different races, strains, or breeds, one did something that was difficult to put in words, but the result was what had been best described as an "epidemic" of variations. In the human family there was a constant mixing up in all parts of the world, and the result was always a certain amount of variation. Hence he thought that the particular condition of things which predisposes or is favourable to the growth and development of the germ of tubercle might appear in any country, and might go on reappearing for ever.

Then coming to the question which ran through all this discussion—the influence of the surroundings or environment on the germ-cells, he understood that Dr Clouston contended that Weissmann admitted that the germ-cells might be altered by environment. He was quite sure of this, that Weissmann holds to the continuity of the germ-plasm; that the power to vary we find in the higher animals was acquired originally by their remote ancestors; and that the variation required to produce new types is obtained by what he calls a "reduction" of the germ-plasm, *i.e.* by a part of the nucleus of one germ-cell (the ovum) being got rid of to make room for another germ-cell (the sperm) having different potentialities. He (the speaker) thought Weissmann would not admit that the germ-cell is directly influenced by environment—that the environment is the actual cause of variation.

All the work which the speaker had been doing lately pointed to the fact that the germ-cells are excessively sensitive, but not to the fact of there being any transmission of acquired characters. If, for example, he mated a very young pigeon with an old pigeon, taking those breeds that have peculiar characters such as frills, hoods, etc., the first produce of the practically immature pigeon are all perfectly smooth-headed and smooth-breasted. But the next set of young, when the bird is in good form and feather—has fully reached maturity—may have all the characteristics of the parent. That meant that the first germ-cells are in an immature condition; so feeble or impotent that they are incapable of handing on the highly

specialised characteristics of the parent. Give the germ-cells time and they will become so prepotent that they may hand on all the peculiarities even when the pigeon is crossed with another breed. In the same way if a rabbit that is still immature is put to an old buck—the buck may serve her, but the germ-cells being immature, there is for a time no ovulation. The sperm-cells lie waiting, sometimes for over a week, and the moment ovulation takes place the eggs are fertilised; and in this case the young all take after the male. If on the other hand a doe is served, not at the right time, but a week or ten days after, when the next young come they are all exactly like herself. In the first place the eggs are hardly ripe, in the other they are over-ripe. He might give many instances to show how much the maturity and ripeness of the eggs determines the condition of the adults developed from the eggs. This was of course altogether different from Weissmann, and he thought it would, when thoroughly understood, enable those who cannot live without the transmission of acquired characters to feel happier.

Professor Hamilton had, all through his paper, escaped the fallacies which had so long prevailed; but until others had reached the same stage there was not much prospect of progress.

He might, perhaps, say a few words on another subject, viz., reversion. It had been recently alleged that there was no such thing as reversion. The subject was originally discussed in a scientific way by Darwin, who thought that reversion had resulted from a sort of antagonism. That reversions occurred the speaker had no doubt. By crossing a pure white fantail having thirty feathers on its tail with a cross-bred pigeon, he had obtained a slaty-blue bird with only twelve feathers in its tail, which closely resembled the wild blue rock pigeon.

In the same way he had evidence of reversion in the Equidæ. In his zebra hybrids he expected to get something between horses and zebras, but had got something in its colouration very unlike either a horse or a zebra. The question arose—was it a new creature or an old creature? By making careful investigation as to stripes, etc., he came to the conclusion that his hybrids in their markings were a restoration of an extremely old type of horses. This had been verified to a certain extent by the discovery in Tibet recently of ponies almost as striped as some of the zebras. The Himalayan ponies and certain ponies found in Norway had characters which agreed in a general way with the hybrids—so that in these hybrids he believed we had again a very marked instance of reversion.

Remarks by DR J. W. BALLANTYNE.—He said that he might best begin his remarks by referring to one point in Professor Hamilton's communication with which he could fully agree, namely, his disbelief in the potency of maternal impressions to cause conditions in the fœtus resembling the impression. At the same time he thought it might be said that to whatever extent we believed the mind capable of influencing the state of a part of the body, to that same extent, or to a degree rather less, the mother's mind might influence her parasitic growth (*i.e.* the fœtus *in utero*). But this amount of belief would of course vary very much in accordance with the elasticity of our belief regarding the influence of the mind over the body. Personally he lamented very much the intrusion of the subject of maternal impressions into biological problems, and more especially into antenatal pathology; it had kept that subject back, and had spoiled many case records which would otherwise have been most valuable.

Having agreed with Professor Hamilton on this point he was now forced to differ from him on two others. He did not think that we could separate off clearly the diseases of the fœtus due to intra-uterine infection from the hereditary diseases. Take first the case of small-pox, about which there could be no difference of opinion. When a pregnant woman had small-pox her fœtus *in utero* might take it. Manifestly the fœtal variola was not passed to the fœtus by heredity. Take another condition which could not be so easily divided off—fœtal tuberculosis. He thought there was no doubt that fœtal tuberculosis truly occurred, but very rarely; it was not to be expected that it could occur often, for the fœtus cannot be infected through the air (*i.e.* by the lungs) but only through the umbilicus; and as it was seldom that the tubercle bacilli were in the mother's blood, it was seldom that they reached the fœtus. Cases of fœtal tuberculosis had been reported, and he thought that (*e.g.*) the one described by Auché and Chambrelent formed an absolutely complete proof. In that case the mother had generalised tuberculosis, and tubercle was found in large amount in the fœtus in its liver and spleen (just where it was to be expected, considering that it entered by the umbilical vein), and to a small amount in the lungs. It was found also in the heart, in the form of tubercular endocarditis. Now, here was one of the points which he wished to emphasise: heart disease was one of the conditions admitted to be sometimes transmitted hereditarily. A mother with tuberculosis might give birth to a fœtus not with tuberculosis but with a cardiac morbid condition. He was quoting Hanot when he said that it had been advanced that the offspring of tubercular parents might escape tubercle by having

a cardiac malformation or a congenital condition other than tubercle; it has been said that in this way the child might get rid of the heredity of tubercle. Was it not possible, however, that this was a case in which the disease was not transmitted from mother to fœtus in the same form, for it did not follow that a mother must transmit the disease to her fœtus in the same quality as she had it herself? She can and does transmit something to the fœtus which is not exactly the same as she has herself. For instance, there was Bidone's case of erysipelas in the last days of pregnancy: the mother had it showing itself in the usual way in inflammation of the skin, but the fœtus was born without any signs of erysipelas in its skin, but it had endocarditis and on the valves of the heart streptococci were found, and streptococci were also discovered in the placenta and in the mother's skin. Could it be doubted that the erysipelas in the fœtus was due to the streptococci, and took the form of endocarditis instead of dermatitis? To carry his argument a little further, it might be maintained that all the things he had named were not *hereditarily* transmitted but by infection; but could any line be drawn between things so transmitted (taking into account the peculiarities of the fœtus and the change in the nature of the handed-on morbid process caused thereby) and the transmission of toxines which did not produce any special disease but simply made the fœtus less well nourished and more likely to take infections?

To go a step further, the embryo might be cited. The modifying effect of environment on the development of the embryo was one of the most clearly established of the phenomena of teratology; malformations could be thus experimentally produced (those at any rate characterised by arrest of development). Toxines of various kinds, hydrocyanic acid very powerfully, nicotine, the alcohols in the degree of their toxicity, these when injected into the hen's egg caused teratogenic effects. In the human subject there was evidence of the clinical kind to support this, for malformations were found more commonly in the descendants of individuals who had saturated themselves with alcohol or had suffered from lead poisoning or other toxic condition or from the infections. Now many of these things were transmitted hereditarily. So he had passed in his argument from small-pox, which no one regarded as transmitted by heredity, through a series of connecting links to conditions such as malformations which were in many cases markedly hereditary in their transmission. Further, the malformations agreed with the neuroses and with gout, rheumatism, and other things, in obeying the same laws in heredity, such as family prevalence, dissimilarity, etc. These laws applied to the various morbid

phenomena of antenatal pathology, viz., malformations, monstrosities, prematurity, dead-birth, congenital debility, twins, etc. So the first point in which he differed from Professor Hamilton was that he thought it not possible to draw a sharp line between what everyone admitted to be a fœtal disease (an intra-uterine infection) and a hereditarily transmitted condition.

In the second place he differed with respect to the value of the experiments disproving the transmission of acquired characters. He thought that so far they had not been tackling this subject in the scientific sense, for they had apparently always begun with the idea that the character to be acquired must be acquired after the birth of the individual. That did not seem to him to be the proper scientific way of looking at the subject. The individual lives before he is born, and it may be that experiments failed to give positive results because they (*e.g.* mutilations, etc.) were begun after birth. To test the matter fairly, ought not the processes to be commenced while the embryo or fœtus was still in the uterus of the mother-animal?[1] If this were done, positive results might be looked for. But apart from this there were Kohlwey's experiments on pigeons: he cut off the posterior digit of the foot, and the mutilated bird got in the habit of turning the fourth digit backwards and using it in perching; he got no descendant of these mutilated birds without a posterior digit, but he got a descendant of one of the pairs with its fourth digit turned backwards like the first. The mutilation was not transmitted, but the physiological adaptation to meet it was.

How were we then to get at this problem of heredity, for there was no possibility of avoiding it? Attempts had been made, so to speak, to outflank its position, and the solution of heredity had been sought among the unicellular animals. It had been attempted to circumscribe the field of inquiry to characters transmitted by sexual generation, and the problem had become only more complex. He thought we must just keep on driving straight ahead, passing towards heredity through the series of antenatal morbid phenomena which lead up to it. In that way one would get back to the germ, and by considering in turn the various links he thought that at length a solution might be found. In conclusion he might state that to his mind two of the special problems of heredity were most inscrutable: one of these was telegony; the other was expressed by Montaigne in the sixteenth century, when he puzzled over the riddle of why he should develop a stone in the bladder at the age of forty-five as a legacy from his father who, however, only

[1] As Charrin and Gley have done with blue pus and rabbits.

developed the same thing when sixty-seven years old, or twenty-five years after his son had been born.

Remarks by Professor STEWART STOCKMAN.—He would only take up a few minutes. In the first place, with regard to tuberculosis. Not so long ago, when he was a student, he was taught to go into a byre, look at a cow, and say whether she was likely to become tuberculous. Since the introduction of tuberculin it was now easy to diagnose what are called occult lesions. With the help of tuberculin it had been found that really these cows were not predisposed to tubercle—they had already got it. He had often wondered what the effect would be of submitting the human race to the tuberculin test. He thought a great many of those individuals who were looked upon as predisposed would be found to be really tuberculous.

As regards congenital tuberculosis, his opinion corroborated that of many others. Nearly every week for the last eight years he had gone with his class to the slaughter-house to examine diseased organs and carcasses; and he had been on the look-out, and the inspectors had also been on the look-out, for cases of congenital tuberculosis.

They in Edinburgh slaughter something like six thousand calves a year, and he had found only one case of tuberculosis among them that was undeniably congenital. Of the cows slaughtered, at least twelve per cent. were tuberculous. This was a low percentage, because they were so strict in Edinburgh that most of the doubtful cases were sent to be slaughtered elsewhere. The general average would be about twenty-five per cent., but in some herds it mounts to eighty per cent. of the effective.

Now, if you take the bullocks and heifers, the tuberculous animals do not amount to one per cent. Surely, if even a predisposition were transmitted, it ought to be to the bullocks as well as to the cows. The question was really one of external predisposing causes.

With regard to the so-called predisposition to tubercle, there was one point to which he thought too little attention had been paid—the influence of heredity, not upon the patient, but on the tubercle bacillus itself. The bacillus taken from one species often fails to affect another. For example, they all knew that it was very difficult to give human tuberculosis to fowls, but occasionally a fowl may be inoculated successfully. It has been shown lately that if the tubercle bacillus from a human being be grown in glycerine broth, put in a collodion capsule, and placed in the abdominal cavity of a fowl (the collodion prevents phagocytes

acting on the bacillus), it after a time acquires the characters of the avian tubercle bacillus, and in the course of some months you obtain a variety which is deadly enough to the fowl.

He had inoculated horses and donkeys with different kinds of tuberculosis, and others had done the same. The first impression was that the horse was very insusceptible, and the donkey absolutely immune. The horse is now known to be susceptible, though no one had ever pointed out a horse that was likely to become tuberculous; the study of equine tuberculosis dates from the discovery of the tubercle bacillus. If one take the bacillus from the cow and inject it into the donkey, lesions are produced, but these are cured in about a month; and in his first experiments he thought the view was quite correct that the donkey was not susceptible. It happened, however, that one day the tuberculous spleen of a horse was sent him for examination. With material taken from this he inoculated a donkey, and the donkey died of tuberculosis in the ordinary way. He thought that when talking about one species being predisposed and another not, we should remember that if the bacillus does not act on this or that species, it may not be due so much to the species as to something in connection with the bacillus.

As regards cancer, he had never heard it seriously said among veterinarians that it was hereditary. Yet they saw a good deal of cancer. No one, however, had observed a series of cases in respect to its being hereditary in animals. Glanders also was a disease which affected horses. In its course it was rather like tuberculosis. It was now known that it often ran a very chronic course, and that the lesions might remain for a long time occult; but it had never been seriously said that it was hereditary. When he heard Professor Hamilton express the view that there was no hereditary tendency in the case of syphilis, he thought he would like to have it explained why this should be so in regard to syphilis and not with regard to tuberculosis?

Lastly, there was one nervous disorder in horses, termed "roaring," *i.e.* paralysis of the left recurrent laryngeal nerve. It was a rank heresy among the majority of horsey men to say that roaring was not hereditary, and yet there were far more tangible explanations of this affection. The famous horse "Ormond" was a roarer, and his sire was a roarer. He was sold for twelve thousand pounds because he was of a roarer strain. He has been sire to a number of colts in this country. Some have become roarers, others have not. It has been said by veterinarians that in such diseases as strangles, influenza, and suppurative diseases of the throat, the bacterial toxins may

produce a condition of the nerves which results in roaring. There are also other good explanations of the condition. In the Argentines, where Ormond first went, it is said that none of his progeny have become roarers. Evidently the hereditary influence plays only a feeble part, if it play any at all.

Dr JAMES observed that it had been stated by a great man many years ago that while philosophers and statesmen were cogitating as to how the world should be governed, hunger and love were performing the task. He thought it very fortunate indeed that humanity out of instinct and common sense should know how to conduct a good life without getting any assistance from scientists in the way of physiology. It was also a good thing for humanity that it could go on reproducing itself to good purpose without scientific aid as regards heredity. He said this because within the last few years he had heard a great deal coming from scientists which was against instinct and common sense, and it would be very bad for humanity were it to adopt these dicta practically. He referred in this connection especially to the statement that acquired variations cannot be transmitted, and to the statements that we have heard lately that much less importance should be placed upon heredity than common-sense people are inclined to allow.

Of course they knew that according to the Weissmann theory acquired variations could not be transmitted. But although Weissmann had begun by saying this, when they looked into all that had been written by him subsequently, they could not but conclude that what he had said after all was nothing very different from what had been said before him. Weissmann seemed to regard a variation rather as a development of something that was there before, than as the occurrence of something new. But is it not the case that evolution means simply the better adaptation of the organism to surrounding conditions? Hence in every generation a new step in progress is, as it were, made. Dr James remarked next that he was not going to follow Weissmann in his metaphysical processes, but he would just quote a sentence which he remembered to have read in one of Herbert Spencer's books. At least he thought he had seen it there. This was that, amongst living beings, the conditions which favoured growth and development were precisely the conditions which, given time enough, would bring into existence.

As regards examples of the non-transmission of acquired variations which had been brought forward, he could not help saying that to imagine they proved anything was simply childish. For example, as regards mutilations.

Certain tribes of Indians had for years, by pressure in early life, flattened their heads. It was said, "Why was not this perpetuated?" The Chinese women had for years distorted their feet. Why was not this perpetuated? Well, in the first place, the years during which those mutilations had been going on were as nothing compared to the duration of life of human beings on the surface of the globe. But further, and he thought that this was the better answer, Nature was not a fool, and Nature, in order to enable human beings to rise in evolution, brought about the elimination of the unfit as well as favoured the survival of the fittest. Nature was not such a fool as to perpetuate flattened heads and distorted feet, but had eliminated or would eliminate the people who practised those mutilations.

Brown-Sequard's experiments on the epileptic guinea-pigs, and the observations on pigeons which Dr Ballantyne had quoted, were, if corroborated, of value as against the Weissmann theory. But Dr James contended that laboratory and experimental work were not likely to give valuable data in connection with the transmission of acquired variations. At any rate, as yet he held that they had not afforded much information. On the other hand; Dr James held most emphatically that doctors in practice had much better opportunities of forming an opinion on this point. Doctors had opportunities of examining for themselves, and of tracing variations in a way that the laboratory scientist never had. He remembered hearing it said by one of his personal friends that, at a recent meeting of biologists, all had declared in favour of Weissmann's theory, except a few doctors. His contention was that the doctors were probably the individuals best capable of judging.

He wished now to explain what he meant.

In medicine it is an old idea—and he thought there was a great deal of truth in it—that the immunity which an individual acquires as the result of having a disease such as typhus, scarlet fever or small-pox, may be transmitted. Personally, he could say that the most malignant examples of small-pox and scarlet fever which he had seen were in the Edinburgh Infirmary, in people who had come in from the out-lying districts of Scotland, where these diseases had not been seen for generations.

As regards syphilis it had often struck him that some of their surgical friends have opportunities which should not be neglected. A man has had syphilis and has been carefully treated. His offspring do not get syphilis—but are those offspring less easily acted on by the syphilitic virus than ordinary individuals, or are they not? We know if a man has acquired syphilis—though the wife may not get the disease—his child may be born

syphilitic. The mother in this case is immune (Colles' law), and may not such immunity be transmitted?

Again, do those individuals whom we see presenting the marks of congenital or hereditary syphilis take the disease if exposed to infection in as virulent a form as those who have not had it in this way?

Next, as regards alcoholism. Dr James was strongly of opinion that alcoholism in a parent produced diseases of a good many kinds in the children in this country. Of course it might be said that the alcohol which the parent had taken had simply brought into prominence what was there before. He was not going to enter into this precisely, because he believed it could do no good, but he held that whether the predisposition to alcoholism was in the parent or not, the practical indulgence in it was very injurious to the offspring. For example, he had, in looking up some of his case books, gathered together some statistics in connection with two diseases, epilepsy and phthisis. In order to limit himself to heredity, he had taken only the epileptic cases under twenty years, and the phthisis under thirty. He found, curiously, that a phthisical family history prevailed in the same proportion in the epileptic as in the phthisical cases. But he had no hesitation in saying that in a large number of the epilepsy cases in children in which it was stated that the parents were quite healthy, one parent, usually, of course, the father, was often found on inquiry to have been alcoholic. Again and again he had found that an alcoholic parentage with perhaps a fall or an injury to the head in childhood, had produced epilepsy, and he looked upon the epilepsy in such cases as an evidence of the transmission of an acquired variation.

Another disease which he had looked into was gout. An insurance friend of his had once informed him that the heredity of gout had been much over-rated, because it had been found that individuals who had been rated up for a family history of gout, had proved to be specially good lives. Now, at first sight this might seem to detract from the importance of heredity, but when we look at it properly, it proves rather how prone we are to draw false conclusions. That individuals with a family history of gout should often prove to be the best lives we can quite easily explain. A gouty heredity means specially good digestive and assimilative powers; and specially good digestive and assimilative powers very often means that organic sensation of well-being which is apt to lead to excess. As Sydenham said long ago, "More wise men than fools have suffered from gout." Now, the men who insure their lives are usually men who have, and can exercise self-control. Hence a gouty heredity in connection with insurance means specially good

digestive and assimilative powers, plus the self-control that prevents excess. The statement, then, that heredity is of no importance, because lives rated up for gout often prove the best lives, is a false one. What we should say is that heredity is important, but that we must make certain that we are interpreting its data correctly.

Remarks by Professor SCHÄFER.—He would content himself with saying a word or two on certain special points on which he had been appealed to. With reference to the complexity of the nerve-cell of course he agreed entirely with Dr Clouston as to its complex structure, but that fact he thought rather accentuated Professor Hamilton's position. No doubt with the early maturity of the nerve-cell it was hardly conceivable that it would transmit any acquired character. He was quite sure that Professor Hamilton would admit that the discussion was to a certain extent a groping in the dark, because we were completely ignorant as to the causation of disease except such diseases as were due to parasites. He thought he was right in saying that we knew nothing as to the actual causes of other diseases. It was, therefore, very difficult to say what they were dealing with when they talked of heredity in disease.

One word with regard to Dr Ballantyne's proposal to make experiments on the fœtus *in utero*. Of course he must be aware that such experiments have been and are being made at the earliest possible stages of development. With frog embryos, for instance, many experiments have been made on the first cells produced from the egg. (Dr Ballantyne agreed that this was being done: he understood with positive results as to the transmission of acquired characters.) The speaker only wished to point out that there was no difficulty in experimenting upon the earliest stages. As to the guinea-pig experiments of Brown-Sequard, he might say that, at the suggestion of the late Mr Romanes, Dr Leonard Hill made a certain number of similar experiments in his (the speaker's) laboratory in University College, but so far as they went it was not found that the guinea-pigs in which this neurosis had been artificially produced transmitted it to their offspring.

Professor HAMILTON, in reply, said he was sure that Dr Clouston must have had a large experience of cases bearing either for or against the question of the acquisition and transmissibility of mental diseases, and from the tenor of

his remarks he was led to believe that he favoured the view that mental disease, in the ordinary sense of the term, may arise from external agencies and be transmitted. With regard to the Jewish types, he was quite right—anywhere on the Continent you might see different types of Jews—but anyone who had had sufficient experience could pick out these to be Jews. One inherited Jewish characteristic is the mental proclivity to commerce which has continued for all time. There are also bodily characteristics in the way of the thick under-lip, and a peculiar conformation of the legs which mark them off.

With regard to Professor Cossar Ewart's remarks, he was pleased to find that, looked at from a biological standpoint, what he had said had been so orthodox ; he had been prepared to hear his ideas characterised as extremely heterodox.

Referring to Dr Ballantyne's remarks about congenital tuberculosis, there might be an element of truth in what he said. It was a well-known fact, which he had verified all his pathological life, that a person suffering primarily from valvular disease of the heart seldom, if ever, died from pulmonary tuberculosis. What the explanation was he did not know.

There was certainly a difficulty in separating the congenital from the hereditary, but he thought we might say that a condition is hereditary when we find it pass through generation after generation ; not merely through one generation, or even a couple, but where the tendency is inherent, like the colour of the hair, or shape of the nose, though perhaps skipping a generation now and again. Of course, the infection of a child with syphilis while in the uterus is not heredity, and he thought that perhaps Dr Ballantyne might have misunderstood what he had said regarding this.

Touching upon the early experiments *in utero* advocated by Dr Ballantyne, Professor Schäfer had in a manner anticipated what he had to say in reply. The beautiful experiments on merogony carried on by Roux and Delage upon the ova of frogs and echinoderms might be mentioned in this connection. These experiments went to prove that the ovum might go on developing into a perfect larva, even though a number of the early blastomeres had been removed ; it was only occasionally that any deformity was met with. Even this radical method of applying external agency had comparatively little influence on normal development.

Professor Stockman had contrasted the difference in frequency of occurrence of tuberculosis in cows and bullocks. He would point out that cows were under different conditions from bullocks, which, it might be, predisposed the former to the infection. The cow was kept in a warm, close, stuffy atmosphere,

while the bullock might be, for a considerable time, in the open; and at the same time, the cow, through excessive discharge of milk, gives off from its body a large amount of nourishment likely to impoverish its system.

On the alcohol question in relation to insanity, there were no doubt a number of fallacies. We might group with the alcoholic habit the tendency to criminality and to insanity, and regard them as manifestations of the same dyscrasia. He thought the tendency to alcohol might be looked upon as an effect rather than a cause of insanity.

Note.—The foregoing reports were corrected in MS. by the various speakers.

The following are some of the speeches which members were disappointed in not having been given an opportunity to deliver. —ED. *S. M. & S. J.*

UNDELIVERED SPEECHES

Remarks by Dr WILSON, Mavisbank.—For the sake of brevity one's statements must appear dogmatic; but our special effort ought to be to keep an open mind on this question. Most of our difficulties are of our own creating, because we have not taken care to translate biological propositions in terms of physiology and of pathology.

The idea of continuity of germ-plasm is not strange and it is not relevant. All living substances are as "continuous" as the germ-plasm until they cease to be; that is, they come in direct line from ancestral substance. The essential substance of germ-cells, however, is continuous forwards as well as backwards and (on paper) is *ad infinitum*. Weismann contends that somato-plasm, the body substance, is not so, that it does not "continue" into our offspring. That may or may not be true —a question which we may waive for the present. This perpetual idioplasm idea is beside the point. The relevant idea propounded by Weismann is, that the germ-plasm which organisms convey in their germ-cells is physiologically secluded; that it is essentially immune to its environment. The germ-plasm, we are taught, undergoes no essential metabolism until the time of maturation. That *may* be so. The characteristic energy of germ-plasm *may* be quite latent for a time. It *may* be that all that germ-plasm requires for its preservation is moist warmth, just as wheat is latent for months in dry warmth. And it *may* be that, every little while, germ-plasm maturates automatically and becomes manifest in the kinetic

energy of the spermatozoa and of ova. All these propositions are still open to grave question. But, at all events, this notion of physiological isolation of the germ-stuff, and of a periodic metabolism in it of an automatic kind, is quite apart from the idea of "continuity."

Weismann admits, and everyone must, that disease has some effect on germ-plasm. The katabolism of somatic death stops the germ-stuff conveyed in our bodies. And death is a relative term. Short of complete somatic death, various degrees of change occur which affect the germ-plasm—degrees of hyperpyrexia, for example, and degrees of toxicity of lymph. But we must state the question at issue more exactly. Physicians have been at fault in not discerning variations in disease. For example, when a man with tabes begets a child with epilepsy, the physician is too ready to speak of the hereditary factor in the child's case. Many similar instances will suggest themselves—cases, not of heredity, but of variation in disease. This idea is important. Diseased organisms are apt to breed disease, but not always, though sometimes, their own disease.

We must relinquish the idea of a hard and fast line between "acquired" and "idiopathic" diseases. All diseases are to some extent "acquired"—occasioned as reactions to environment. In some cases the agencies within the organism are of more obvious importance than in others. Let us take a case in which both factors are important—a case of tuberculosis. To obviate the fallacy of maternal infection, suppose the disease to be of the avine variety, and that the yolk is not infected. Assume that the last egg of a fowl dying from tuberculosis is fertile. Weismann would admit—everyone would—that the chick is likely not to be full-grown and robust. It will fail of "nutrition," of a full capacity for regeneration, and of normal resistiveness to environment (terms which require fuller consideration). It would appear then that this chick has an idiopathic susceptibility to all and sundry, or at least to several, diseases. But it is mere slackness to call that heredity in disease. It is equally apt to be variation; the chick turning out to be epileptic, or deformed, or liable to cholera. That is all that Weismann contends for. The disease has not bred itself. And when we come to consider human diseases, and have regard to all the possibilities of infection, we must admit that there is no proof that "acquired" diseases ever become "idiopathic." Yet, with others, I venture to believe that the last word has not, by a long way, been said upon this subject. Idiopathic diseases are, for the most part, due to premature or focal arrests of development, or excesses of development or of growth, or senility or involution in the tissues. And we may with reason hold that it is *not proven*

whether, for example, an arrest of cerebral development by alcoholism does or does not induce a similar arrest (among others) in the offspring. The idioplasm of the germ-cell nucleus, as Weismann conceives it, is a molecular substance. The exigencies of debate make it necessary for biologists to whittle away the conception, and it is a fair retort to say that they are hiding a theoretical unit. Weismann would himself admit that the germ-plasm and its determinants are mystical. All delimitations of tissue, including the conception of the cell as unit, are arbitrary. There is no permanent truth, but merely convenience, in the conception of a unit and its environments. The whole organism and all nature are but parts and the relations of parts. If we push the biologist to extremes, he must eventually define the germ-plasm as that substance (in or near the germ-cell) which is independent of its surroundings. That is simply to conceive a substance as capable of what the argument requires. But physiologists will be slow—and philosophers will refuse—to entertain the conception of a living substance which has no relations.

The invaluable contribution of Weismann to the practice, as well as to the theory, of medicine still remains—that we must look to the environment, and to the reactions, of the idioplasm to explain disease and to prevent it. That should be part of a general conception. Taking for the moment, any one organism as unit, its environment is undergoing selection, and is evolving, much more obviously and rapidly than its idioplasm. It is in the selection and in the evolution of environment that the future of medicine lies.

Remarks by Dr W. LESLIE MACKENZIE.—Professor Hamilton's paper appeared to me an admirable application of the doctrine of natural selection to the problem of inherited disease. On one point—telegony—his propositions outran the evidence; for Professor Cossar Ewart has shown, in the Penicuik experiments, that the supposed proof of telegony does not stand analysis, and experiment has not yet furnished a single confirmation. The discussion, it seems to me, somewhat lost sight of the precise issue so clearly raised by Professor Hamilton, namely,—Is a disease that has been acquired by the individual capable of being transmitted as such to his offspring? Toxic diseases, which are capable of infecting the germ-plasm directly, are, of course, irrelevant to the question of inheritance. Why is there any difficulty in answering the question? Mainly because the two—or more—sides to the dispute did not make explicit their fundamental assumptions. Both sides, I think,

admitted the abysmal difference between a somatic-cell, which is capable only of producing its like, and a germ-cell, which contains in itself the elements of the whole body. No one seriously disputed the propositions that the material of the germ-cell, more properly the germ-plasm, is continuous directly from person to person, and that the non-germ-plasm is not thus continuous. Further, it was assumed that the germ-plasm is the bearer of all that the child inherits from the parent. Once more, it was assumed that all the characters existing in the germ-plasm that the parent sprang from may be transmitted to the offspring; that some of the characters may lie latent; but that, on account of the continuity of the germ-plasm, these may be handed on to a third and a fourth, and an nth. generation. Then all appeared to assume that if the germ-plasm varied in any way, natural selection will at once operate to develop and preserve useful variations. One assumption, however, did not seem to be constantly present to the minds of the disputants, namely, that there is a fundamental distinction between a variation (however produced) in an individual's germ-plasm, and a modification (produced by use, or environment, or disease) in the body (that is, the non-germinal-plasm) of the same individual. The variation in the germ-plasm will result in the production of a new character in a new individual springing from that germ-plasm. But a modification due to use, or environment, that is, a character acquired by the individual, will not be transmitted to his offspring unless it first produces a definite variation in the germ-plasm. Darwin, assuming as a fact that characters acquired by an individual were transmitted, invented pangenesis to show how the transmission was possible. But Galton found reason to believe that pangenesis, that is, concentration of representative gemmules in the germ-plasm, was mythical, and also found reason to doubt whether acquired characters were inherited to any but an extremely small extent. Weismann drove the analysis further, and not only showed that many acquired characters supposed to be inherited were not inherited, but that the thorough-going application of natural selection made the hypothesis quite unnecessary. Many difficulties exist on both sides; but in the problem of hereditary disease, it is, I submit, illegitimate to assume that the inheritance of acquired characters has been proved. On the contrary, it remains entirely unproven. So far, experiment has failed to make it even predominantly probable. By "acquired" is to be understood "acquired by the parent during his own life-history." From conversation, I gathered that some understood "acquired" to mean acquired by the germ-cell. The germ-cell, or germ-plasm, is an organism within the body-organism. Like cell

organisms, it depends on its environment (the body) for nurture; but nurture affects function and growth, not structure.

Consequently, it is a very "cheap" and, only in appearance, a very simple assumption to say that any structural modification in the germ-cell's environment (the body) will create in the germ-cell such an alteration that when the germ-cell grows to an adult, that adult will have the same structural modification as the parent. But to judge by the instances produced at the discussion, the inheritance of modifications acquired by the parent is a superfluous assumption. In speaking of Weismann's "admissions," Dr Clouston left on one the impression that he considered the admissions somewhat inconsistent with the fundamental assumption of the continuity of the germ-plasm from generation to generation. But when Weismann admits the "transmission of disease," he means such diseases as may infect the germ-plasm; but, as Prof. Hamilton showed, the direct infection of the germ-plasm by a poison circulating in the body is quite irrelevant to the question of inheritance. Weismann would not, I take it, admit that (say) cardiac hypertrophy due to over-work would produce congenital cardiac hypertrophy (or even a tendency to it) in the child. Of course, the constitution that made the father liable to hypertrophy would also make the child liable, but this is inheritance of a constitutional (non-acquired) character—a thing no one disputes. Then "the laws of transmission do not affect the lowest forms" is said to be another admission. I assume the general accuracy of the statement. The explanation is simple: in the unicellular organisms there is no division into germ-plasm and body-plasm. There is, consequently, no heredity in the sense applicable to higher organisms. In the lowest forms the whole body of the parent divides into two daughters. But, in the higher, only a minute fraction of the organism is set apart for reproduction.

Again, a third admission is that "the environment, as climate, has the power to alter germ-cells." Prof. Cossar Ewart corrected this statement, which certainly does not completely represent Weismann's doctrine. The word "alter" is ambiguous; it may mean alter functionally, or structurally. The germ-cell is, of course, affected by its own environment, as I have said above; but Dr Clouston seemed to indicate his belief that climate might produce in the germ-plasm directly elements that did not before exist in it, so giving rise to new variations in the germ-plasm. This proposition needs a vast amount of proving. I have not found Weismann maintaining it, but he admits that, in certain butterflies, climate may stimulate the growth of particular elements of the germ-plasm—a very different thing. None the less, Dr Clouston is, I think, right in

concluding that Weismann's hypothesis includes everything essential to the explanation of hereditary insanity. I felt, however, that in speaking of the "royal family insane for three hundred years," Dr Clouston seemed to suggest the inheritance of acquired characters. Did he imply that the "psychopathic constitution" inherited by this unfortunate family was first produced in an ancestor three hundred years ago, wrought somehow—by pangenesis, or other unexplained mechanism—into the structure of his germ-plasm, and so transmitted to his descendants? If yes, then I say this begs the whole question. If no, then the "psychopathic constitution" was not an acquired character, but a character already existing in that ancestor's germ-plasm. He transmitted the constitution he was born with; but that brings us no nearer an explanation of how he came to be born with it. But, incidentally, the stability of this dyscrasia in the germ-plasm for three hundred years on the whole confirms Weismann's doctrine of the continuity of the germ-plasm. Again, Dr Clouston strongly maintains that many of the insanities are manifestations of arrested development. But Weismann never denied the possibility of arrested development, which may arise from a thousand causes other than inheritance of acquired characters. But Dr Clouston naturally asks why so many bad variations occur in the same family? This problem of the occurrence of variations that have selective value is tackled by Weismann in his "Germinal Selection," and most of the difficulties seem to me better correlated by that formula than by the doctrine of use-inheritance. In any case, even if use-inheritance were accepted, how can the transmission of so useless a variation as insane neurosis be accounted for? Dr Clouston says he does not believe that the child of an alcoholic parent is ever born with a taste for whisky; but he may be born with a lack of inhibition. But such lack of inhibition may very well have existed in the parent's constitution before alcoholism asserted itself; and, in any case, the nutrition of the germ-cells may be interfered with by the alcohol circulating in the parent's body, and germinal decrepitude may thus be produced. It is even *possible*, theoretically, that the nerve-elements of the germ-plasm (the nerve-determinants) may be so affected by alcohol that they grow into imperfect neurons or nerve-cells. But here again it is a case not of the transmission of characters acquired by the father, but of the effects of alcohol imbibed by the germ-cell itself. The case is on the same footing as a case of syphilitic infection, with this difference, that the poison of syphilis is a specific infection. The results of infecting or poisoning the germ-plasm may be anything from a temporary "illness" of the germ-cell to complete disintegration and death. The poisoning

of the germ-plasm, however, has nothing to do with the transmission of a modification acquired by the parent. Other illustrations by Dr Clouston seemed equally explicable from Weismann's standpoint. He produced no case that contradicts the continuity of the germ-plasm or necessarily involves the inheritance of acquired characters. On the contrary, his cases are more simply explained without that assumption.

Dr James' precise position I was unable to grasp. I gather that he asserted the inheritance of acquired characters as too obvious to need discussion. But his illustrations—alcohol, phthisis and epilepsy—did not bear out his argument. Professor Hamilton's suggestion that these—and others—are all parts of the same constitutional dyscrasia seems to be admitted by Dr James. Why he should burden himself with the obscure and extravagant hypothesis of use-inheritance, I do not understand. Dr James says medical men have much more opportunity than biologists of knowing whether acquired characters are inherited. Possibly, but the characters are usually too complex for analysis. If they are inherited to the extent he assumes, cases ought to exist by the hundred thousand. It is somewhat disappointing that, with the single exception of Brown-Sequard's rough and inconclusive experiments on guinea-pigs and Dr Ballantyne's illustration of a possible case in pigeons, not one clear and criticised example of use or mutilation-inheritance was brought forward by any speaker. There are many examples much more plausible than Dr James'. The only definite biological facts produced (by Professor Ewart) told quite distinctly against such inheritance. As to Dr James' logical canon that what contradicts "common sense," and "common instinct" has something wrong with it, one is provoked to ask, with Cyrano, "*Que diable allait-il faire en cette galère?*" The discovery of the barometer, the application of the laws of gravity to the solar system, the doctrine Galileo was tortured for, the conservation of energy, natural selection, and many other great inductions, contradict common sense, which plays a very small part in the solution of any difficult and delicate problem.

Dr Ballantyne's remarks on "maternal impressions" are valuable, because he has pretty well done for that department of uncritical belief what Weismann did for the alleged inheritance of mutilations. Yet the so-called evidence for such impressions seems to me frequently stronger than the evidence for inheritance of acquired characters. When, however, Dr Ballantyne suggested antenatal mutilation for experiment, as more hopeful, he gave the general case away; for the acquired characters alleged to be transmitted are not the simple conditions induced by the very

primitive environment of the uterine fluids, but the immensely delicate and complex integrations, dexterities, ideas, etc., developed, or *de novo* fashioned, in response to the complex environment of actual life. The case of Montaigne's calculus, which supervened at the same age as his father's, is an excellent example of the appearance of a germinal peculiarity in two branches of the same tree. Professor Schäfer's very important remark should give the "medical transmissionists" pause: infections are, by hypothesis, ruled out, and of the ultimate causation of other diseases we know little or nothing. For practical ends, pathology is an organised department; but biologically, disease is a *plus* or *minus* variation or modification, sometimes having selective value, sometimes not. Personally, I should be glad to believe, with Dr James, that much of these refinements in distinction are merely verbal, were I not persuaded by daily experience that the nightmare of the specific inheritance of acquired diseases overloads the spontaneity of life, paralyses the will, and hampers the preventive service in its efforts to improve the environment. Weismannism exalts the social inheritances, which, as the great organs of selection, constitute the basis of preventive medicine. But there are no data for a dogmatic conclusion on the modes of hereditary transmission of acquired or non-acquired characters.

Remarks by Dr GRAHAM BROWN.—It appears to me that the discussion turns very largely on a play on words. What do we mean by acquired characteristics? Do we mean such as were not present in the ancestry of the person in question? If we do mean some abnormality or divergency from the normal type, some alteration of tissue which may perhaps only show itself by an increased susceptibility to the action of external agency, then I am firmly of the belief that such acquired characteristics may be and often are handed down from father to son for generations.

Take, for example, the case of rheumatism. No one who has had any opportunity of studying this disease, and of acquiring a knowledge of the facts concerning it, can deny that three points at least are clear and well established.

1. That it prevails very much more in some families than in others.

2. That there is no evidence to show that it passes from person to person by infection or contagion.

3. That it is of the nature of an infective complaint.

Taking these points as admitted, it follows, to my thinking, that the occurrence of rheumatism in certain families can only

be due to the transmission of some peculiar susceptibility of tissue from father to son.

Or take the case of alcoholism. As Dr Clouston has so admirably pointed out, an increased susceptibility to the action of alcohol (apart altogether from the alcohol craving) is noticed in neurotic families. This unusual vulnerability of the tissues to the action of alcohol can only, so far as I can see, be an acquired characteristic. I have certainly seen it in process of being acquired. That is, I have seen men who at first were able with impunity to take a fair quantity of alcohol who subsequently became alcoholic, and who having broken off the habit, found that in future even small quantities were apt to produce disagreeable results. The onus of proof lies on those who hold that these two forms of transmitted susceptibility of tissue are not acquired characteristics.

Remarks by Dr WILLIAM RUSSELL.—In the first place, I wish to express the pleasure it is to me and to many others here to see Professor Hamilton amongst us again. In listening to the discussion it has appeared to me that we have suffered from a want of definition in the terms used, and also a want of concentration on what seems to me to be the real point implied in the title of the opening paper. The existence of heredity is not questioned, but we have a biologist like Professor Cossar Ewart speaking as if nothing were to be regarded as heredity, unless you could count it or figure it as number or arrangement of skin markings, the colour of feathers, or the number of them in a pigeon's tail. We are all interested in Professor Ewart's experiments, but most of them have no special bearing upon the question under discussion. The heredity which manifests itself in cross-breeding has only a general bearing on the influence of heredity on disease. We all know of curious hereditary anatomical characters in families: I know a family where webbing of fingers or toes prevails; but all that class of thing is not disease any more than are the colour of the eyes or the hair, or the size of the hands or feet. Another biologist tells us that we do not know what disease is; this is a strange statement, for those of us who deal with disease would say that in diseases not belonging to the infective series it begins as disturbed function, and the limit at which that disturbance begins is determined by the vigour of the cells of the part or organ affected. In the common matter of gastric digestion this is true; it is equally true of heart muscle. This is practically the same point as Dr Clouston so ably elaborated; it is the attainment of a "maturity" which is on a level at least with

the average. This maturity is functional vigour. And will the biologist tell us that he knows an anatomical distinction which will enable us to say that one stomach is $x-$ and another $x+$? And because he cannot do this are we, as physicians and pathologists, to ignore this whole realm of observation as if it were a chimera, a mere dream? If biology is not prepared to take this teaching from us, it is doubtful what practical value biology can be of to us. I unhesitatingly range myself alongside Dr Clouston and say that the future of medicine depends more on this class of observation than on variations in pigeons' tails. The question before us seems to me to be this: (1) Can the chemico-vital activity and vigour of cells be influenced by the environment of the individual; can it be lowered, or modified, or altered? (2) Does that lowering or modification in any way influence the offspring along the same lines? There is surely only a positive answer to these two questions. Doubtless this modification may take generations to become fixed, for fortunately the *vis naturæ* tends to strengthen the weakened and to correct what has been modified, if the conditions are rendered more appropriate, more physiological, more hygienic; call it by what name we like, it means a favourable environment, and includes, as Dr Clouston has said, climate, food and drink. The variations and subtleties of the chemico-vital composition of individuals is vulgar knowledge to those of us who deal with deviations from the normal in living human beings. Take some examples of this. Some people are phenomenally vulnerable or susceptible to infective influences, while others are the reverse. I knew a surgeon who could not make a *post-mortem* examination, or operate on a septic case, without getting poisoned. Many men work for years in the *post-mortem* room, and handle all things freely and are never poisoned. I know a medical man who took infective diseases whenever they became prevalent. He had, I believe, scarlet fever and typhoid several times; other medical men never have had either. Take what we call idiosyncrasy, even in the matter of food. I know a leading physician to whom a certain white fish is an emetic; to an old gentleman whom I have seen professionally, egg, in the smallest quantity and in any form, has always acted as an irritant poison, producing vomiting and diarrhœa; in another case a couple of strawberries produces a general urticaria. Passing from food to drugs, I once made a man as red as a lobster from head to feet by a small hypodermic of morphia. Within the last year I produced in an adult male private patient severe diarrhœa by giving him one or two one-drachm doses of the camphor water of the pharmacopœia. I presume we all meet these curious phenomena. Then if we turn to dyscrasia, are there no rheumatic and gouty people?

Do we not recognise that these people have a modified vital chemistry which, under conditions to us harmless, manifests itself by what we clinically know by these names. In all these regions we are face to face constantly with what Professor Ewart would call an "epidemic of variation," not of coarse and palpable anatomical change, but of chemico-vital composition, which neither the biologist nor the physiological chemist can express even in symbols. Because they cannot do this, are we to regard this whole aspect of life as unreal, and existing only in our medical imaginings? I do not think we can too strongly repudiate any such suggestion.

There is certainly variation enough; how much of it is inherited, how much due to blends between male and female not physiologically successful, and how much acquired, are questions we cannot always answer. That acquired characters can be propagated there is strong proof. It is generally accepted that the two commonest infective diseases in this country, namely scarlet fever and measles, are much less virulent than they used to be, and this is almost certainly to be attributed, not to an attenuation of the virus, or to improved treatment, but to a measure of immunity acquired by a population whose progenitors for generations have passed through the ordeal of these infections. It is a historical fact that measles introduced into a virgin soil, unmodified by a degree of immunity acquired by transmission, proves a most virulent and fatal malady. Another historical instance is that of the company of Esquimaux who, taken to Berlin, were vaccinated to prevent them taking small-pox, and they nearly all died from the modified virus, used without any risk for protective purposes by the ordinary European.

We can now pass to Professor Hamilton's proposition that there is a hereditary anatomical type prone to certain definite diseases, and he illustrated this by reference to the general body type and the chest forms in persons liable to pulmonary tuberculosis, and which are well known. But this type and form constitute but a very small proportion of cases of pulmonary tuberculosis; and even where it does, the question is: Does not acquired tubercle determine the anatomical type in the offspring rather than the other way about? Even granting that the individual factor which allows entrance to the tubercle bacillus be an epithelium of low resisting power in the respiratory tract, is it for a moment to be allowed that that enfeeblement cannot be induced on the one hand, or strengthened on the other hand, by environment? Every physician says "No" to this, and the facts are so conspicuous that the matter need not be argued. While this enfeeblement of epithelium may be postulated and accepted as a working hypothesis, let us be perfectly clear in our own

minds that this is not an anatomical distinction. No biologist or pathologist can say of a bronchial or alveolar epithelium that it has anatomical characters which distinguish it from another. We cannot move without postulating chemico-vital modifications and modifications of metabolic vigour which determine the beginnings of disease. I wholly believe with Dr Clouston that these modifications are acquired and transmitted. At an institution in this city I see a great many habitual alcoholics, and our inquiries show that in the greater proportion of them there is a marked family history; but the cases I see differ from those which come into Dr Clouston's experience, for comparatively few of them belong to the neurotic type. In my experience there is a periodic craving which the victims will do almost anything to satisfy, and which is a pathological condition only stateable as a chemico-vital perturbation, probably in nerve endings, which is transformed into conscious craving in cerebral cells. This tendency must be acquired, unless it is to be traced back to Noah; and even were this possible, the hereditary modification is not present in all his descendants, and must therefore have been eliminated in some by evolution. This morbid reaction to alcohol can, it seems to me, be, without any question, acquired, and it is as certainly transmissible. The same line of observation and of reasoning can be followed in rheumatism and gout —a vicious chemico-vital variation is transmitted, and most of us, I think, assume that the acquirement of this variation need not be thrown many generations back. The transmitted variation determines a great variety of anatomical change and of clinical phenomena, but the vitiated chemistry is none the less a real inheritance, because the coarse anatomical changes are not congenital, but supervene in adult life. To me the whole question of the influence of heredity in disease centres in and radiates from this point of acquired and transmitted chemico-vital variations in cells, tissues, and organs.

NOTE ON LEVURINE

By A. R. SIMPSON, M.D., F.R.C.P.Ed.,
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WHEN I was in Paris during my Christmas holidays, Dr Doyen told me that he had seen some remarkable results from the use of levurine. Madame Doyen was suffering from a carbuncle on the arm, which was beginning to suppurate. Administration of levurine arrested the suppuration and led to speedy subsidence of the swelling. His little boy had

acute pneumonia; levurine brought down the temperature. My son, who had the privilege of working in Dr Doyen's clinique, had a sharp tonsillitis, which was checked by the same means.

Dr Doyen kindly gave me half a dozen bottles to experiment with. I gave one to Dr Struthers Stewart for the benefit of a patient of his, from whom I had removed the tubes and ovaries in the middle of December. She had suffered for ten years from the effects of pelvic peritonitis. Latterly the right ovary had begun to enlarge. The operation was difficult because of the extent and strength of the adhesions. We specially noted at the time of the operation that the appendix vermiformis was bound to the left tube, and after detachment had an inflamed appearance. She was progressing on the whole favourably till January 5th, when the temperature rose to 102°, and she began to show symptoms of appendicitis. It was for this that the levurine was given her, because although the temperature had come down during the night to 100° it showed a tendency again to rise. Under the levurine it came down nearly to normal, and each time it approached 100° a fresh dose was again given with like result. Dr Stewart told me that he has administered it also in a case of old tubercular ulceration with good result.

In the Maternity, J. M., æt. 19, was delivered of her first child on January 14th. The two following days she was well. On the 17th the temperature had risen to 100·8° F. in the evening, and the pulse was 120. There were other septic indications which called for the use of an intra-uterine corrosive sublimate douche and the hypodermic injection of a dose of anti-streptococcus serum. The temperature continued to rise till at 2 A.M. it was 102·4°. It began to come down under a repetition of the intra-uterine douche, but was still at 101·1° next day when a dose of levurine was administered, with speedy relief of symptoms and reduction of temperature to normal. The following day, the 19th, as the temperature was rising and had reached 100·2° in the afternoon levurine was again administered, and again with the result that the temperature was reduced, this time to 98°. The child's suckling had been intermitted when the febrile symptoms showed themselves. But now the nursing was resumed, the puerperium ran

its natural course, and there being no longer any local or general disturbance the patient left the hospital on the 24th.

In another patient in the Maternity at the same time the beneficial influence of the drug was seen. She also was primiparous, but had been for some time under observation, suffering from albuminuria. On January 17th she gave birth to dead twins. The following night the temperature being 102·2° a subcutaneous injection of anti-streptococcus serum was made at 2.30 A.M. The temperature fell to 100·2°; but when it had risen to 101·4° at 3 P.M. of the 18th, the serum injection was repeated. At 6 P.M. on the 19th levurine was administered, as the temperature had risen to 102·3°. A second dose was given at 2 A.M. of the 20th. At 8 A.M. the temperature was 98°. At 8 P.M. it was again up to 101°. Administration of a drachm of levurine brought it down to normal at midnight. At 7 A.M. of the 21st, the temperature being 101·6°, levurine was given once more with again the same good effect. The chart shows further that it was again administered on the 24th, when the temperature had risen; and after that it was not required.

A young primipara who had had a threatening of pelvic cellulitis after her confinement in the Maternity in December, was sent into my ward in the Royal Infirmary, on January 8th, with a cellulitic deposit in the right side of the pelvis. She had a temperature of 104·3°, which came down under levurine to 98·8°. Notwithstanding that the levurine was administered thrice a day the temperature rose again to 104° on the third day after admission, and again on the sixth day to 103·3°, with depressions between, sometimes to 97·8°. On the seventh day it did not rise above 99·5°, when her chart notes that the use of the levurine was stopped, and under the ice-bag, which had all along been applied to the abdomen, the cellulitic deposit has been gradually disappearing and the temperature has not again risen above 100·8°.

Dr Bruce, to whom I had given one of the bottles, writes me: "In the case of carbuncle where I tried the levurine, I thought that the absorption of the inflammatory mass was much more rapid than it usually is in such cases. The case of a fairly large carbuncle, which had been increasing till the levurine was begun, seemed to have in three days almost

entirely gone, by absorption, without material sloughing; and in two days more the surrounding hardness with it. In a similar case I should certainly try it again."

Levurine, some of the virtues of which have been illustrated in these cases, is the designation given to a dried extract of beer yeast (*levures de biére*), prepared by M. Couturieux, chemist, and described by him and Dr Aragon in an article in the *Bulletin Medical* of 5th July 1899. A copy of their brochure, accompanying each bottle, states that they were stimulated to the preparation of the drug by a paper in the same journal in 1897, on the use of barm in diabetes mellitus, in which Professor Agrégé Cassaët, of Bordeaux, and M. Beylot, licenciate in science, had recalled previous observations as to the value of yeast in the treatment of furunculosis. Levurine is, therefore, not a new drug, but the yeast of beer so prepared that we have it concentrated in powder form, always fresh, constant in its action, and in an easily administrable form. It may be taken in beer or aërated water, or, to those for whom the taste is too repugnant, in cachets. The dose is a teaspoonful, to be given before food, three times a day if necessary, until the desired effect is attained of arrest of suppuration and reduction of temperature. Whether the levurine produces its effect by counteracting, as Cassaët and Beylot had suggested, a latent glycæmia, or by acting as a germicide to streptococci and staphylococci, or as an antagonist to their toxines, or in whatever way it works, the results of its administration in the cases I have noted are so striking that I have thought it worth while to bring it thus under your observation.

ALBUMINURIA, MULTIPLE PREGNANCY, PAPYRACEOUS FŒTUS AND PLACENTA PRÆVIA[1]

By JAMES RITCHIE, M.D., M.R.C.S.Eng., Edinburgh.

MRS R., aged thirty-two, was in her earlier life somewhat rheumatic and liable to frequent attacks of gastro-intestinal catarrh and consequent anæmia. She was married in 1895, and on 21st January 1896, she was delivered at term of a healthy girl. She had enjoyed good health during her pregnancy and made

[1] Read before the Obstetrical Society, Edinburgh.

a good recovery. She did not make a very good nurse but continued to suckle for ten months and a half. After weaning menstruation recurred regularly till the commencement of 1899. Her last period began on 28th February. On 22nd July the writer was called to see her in consequence of severe headache, shortness of breath on going up stairs, and debility. The face, feet, legs and thighs were much swollen, urine was scanty and contained a large amount of albumen—it became nearly solid on being boiled, but there were no tube casts and no blood. Appetite was very poor, she was squeamish, headache was severe, little sleep, and the pupils were widely dilated. The blood was not examined for the microbe described by Leoniovitsch under the name of planococcus of eclampsia.

The treatment adopted was rest in bed, the bowels freely moved, a mustard blister to the nape of neck, a teacupful of water or of whey every two hours, no other food, and fifteen grains of bromide of sodium four times a day (in consequence of sickness very little of this was taken). The following day the urine was markedly increased, it contained less albumen and headache was less severe. After a few days the urine became copious and there was such improvement that milk and water were given and later some toast. After ten days œdema was gone, there was no headache, and the albumen in urine was only a trace. A little liberty in diet was allowed, simple milk puddings, tea, and later, white fish. Still later the patient was allowed out of bed, the urine being examined after each change in treatment. After some weeks she was allowed out of doors in fine weather.

On 20th September the writer was summoned because of slight hæmorrhage; this had first occurred three days previously and it had recurred that morning. As it had been very slight, seemed to have quite ceased, and the patient stated that the movements of the child had been felt as usual, no vaginal examination was made, but the patient was instructed to remain in bed and to take food cool. Early next morning the writer was sent for because of sharp hæmorrhage; there was a second hæmorrhage before his arrival. On examination the os was found partially dilated, and projecting from the os externum was a triangular firm wedge of placenta. The pains were good, the

os dilating; as there was no further hæmorrhage, progress was left to nature. About two hours later a macerated fœtus about the size of a five and half months' fœtus was born, and soon after, with almost no hæmorrhage, the secundines were discharged. The uterus contracted well. After careful examination of the secundines it was concluded that they had been completely discharged, there was no appearance of a probable site of a succenturiate portion of placenta. The further history showed this to have been an error, because on the following day a portion of placental tissue about 1½ inch in diameter came away, and at intervals a few small shreds of placental tissue were expelled, with slight increase in the amount of discharge. As the temperature was normal, except on a few occasions when it reached 99·4° in the evening, the uterus was not curetted. A hot vaginal douche was given twice daily, and four drops of tincture of nux vomica four times a day. Mrs R. made a good recovery, although for a few days there was some anxiety because of the discharge of small portions of placental tissue. A few days after confinement albumen was entirely absent from the urine.

After careful examination the placenta was seen to consist of one mass, but not having the same consistence throughout. There was a smaller, firm, exsanguine portion—that which was felt on digital examination—and a larger portion more normal in appearance. The membranes were found to consist of a single chorion with two amniotic sacs—a larger sac, and also a smaller one near to the os uteri, which contained a small flattened fœtus. The umbilical cord of this fœtus had a velamentous insertion; in consequence of this its vitality had been impaired, it had died, and become compressed—a so-called papyraceous fœtus. The portion of placenta which belonged to this fœtus became functionless, exsanguine; it was this portion which presented, and it was because of its exsanguine character that there was much less hæmorrhage than would otherwise have taken place.

For seven weeks before confinement the amount of albumen was small—under 0·25 grm. to the litre, sometimes it was absent; nevertheless, the nutrition of the fœtus was seriously involved and ultimately it died, another example of what has often been observed, that in albuminuria of pregnancy the

nutrition of the child is seriously affected although the amount of albumen in the urine is small. Professor Davis of Philadelphia states that serum albumen in the urine is only of importance when in excess (it is presumed that he means when the quantity is large), and when accompanied by kidney debris. Both of these statements are not in accordance with the writer's experience. In cases showing only a small amount of albumen the results may be serious to the child, even if there be no evidence of organic disease of kidney.

The cause of the occurrence of albumen in the urine is in some cases difficult of explanation. In an investigation made by Professor Sir T. Grainger Stewart some years ago he found that of—

Private patients examined, 24 per cent. had albumen in the urine.

Indoor Infirmary patients examined, 49·3 per cent. had albumen in the urine.

Outdoor Infirmary patients examined, 19 per cent. had albumen in the urine.

Children in the Royal Hospital for Sick Children examined, 14 per cent. had albumen in the urine.

Patients in the Fever Hospital examined, 66 per cent. had albumen in the urine.

Patients in the Maternity Hospital examined, 72 per cent. had albumen in the urine.

Meyer, from a study at Copenhagen, found albumen in 5·4 per cent. of pregnant women, and during the last thirty days of pregnancy he found only 28·9 per cent. of all urine examined free from albumen. Premature births occurred in 8 per cent. of patients with albumen.

During pregnancy, apart from those cases which we still, because of partial ignorance, call "functional albuminuria," and those cases of albuminuria due to organic disease of kidney, and those which are due to heart disease, there are some which are caused by the pregnancy. These cases have been carefully investigated, many experiments have been made and many theories have been advanced with the view of elucidating their etiology and pathology. As yet we only know that in them the albumen occurs not only during pregnancy, but as a consequence of it; that it occurs independently of heart or of

kidney disease; that its occurrence during one pregnancy does not indicate that it will recur during succeeding pregnancies; that it is due to some condition peculiar to the pregnancy involved—possibly some unusual condition in the relation of the products of conception to the mother; that it is associated with the development of an unusual toxic condition of the blood. Although a micro-organism has been described in connection with eclampsia—a planococcus—it has not yet been proved to have a causal relation. The cause of the albuminuria of pregnancy is peculiarly prejudicial to the nutrition of the fœtus, and the severity of the result is not in proportion to the amount of albumen in the urine.

Beyond these facts we are still in ignorance of the pathology of the disease, and therefore our treatment cannot yet be founded upon a secure basis.

When we have to treat such a case we have always to bear in memory that there is a possibility of grave danger ahead—danger to the mother of eclampsia, danger to the child as to its nutrition. For the development of eclampsia three conditions are requisite: (1) a certain quality of nervous system; (2) a toxic condition of the blood; (3) some peripheral irritation. In certain constitutions, especially in children, slight exciting causes produce convulsions, in others under apparently similar conditions, except that of constitution, there seems to be no tendency to convulsions.

Mrs R. is a woman of very highly nervous constitution nevertheless, although her condition when first seen was one of considerable gravity, there was no convulsion.

Although our knowledge of the pathology of albuminuria of pregnancy is so defective, as far as our present knowledge carries us the following principles seem to be indicated:—

1. Reduce the amount of nitrogenous food.
2. Limit the production of toxic material.
3. Favour elimination.

In order to meet these indications rest in bed is important, because it places the skin in a condition favourable to excretion, it also limits the wear and tear, conserves the strength, and at the same time limits the production of waste material. The bowels should be cleared out, and the kidneys attended to. For the latter nothing is better than water or whey.

Digitalis should not be used because in these cases we generally have a pulse of high tension; digitalis increases this and aggravates the headache. Headache, if present, may be relieved by sinapisms to the nape of neck, and sleeplessness by bromides. After a more copious flow of urine is established milk may be used, or milk and water.

Two questions of importance arise in such cases, viz., how long should the patient be kept in bed? and, how long subjected to a purely milk diet? It is a serious matter to keep any one confined to bed longer than is absolutely necessary; it is irksome, the muscles get flabby, and the general health is liable to suffer.

If the urine is copious, dropsy gone, little albumen present in the urine, and a moderately nourishing diet possible, the patient may be allowed some liberty. The result in the urine to be watched. Cautious change in the diet may also be tried, using the quality of urine as an indication as to whether increased strictness or greater liberty should be adopted.

THE DIAGNOSIS OF CHRONIC EMPYEMA OF THE MAXILLARY ANTRUM

By G. HUNTER MACKENZIE, M.D.,
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THE diagnosis of chronic empyema of the maxillary antrum (antrum of Highmorr) is largely dependent upon one sign, viz., the presence of a discharge of fluid pus from the nose by the anterior nares, and it is for the relief of this that the individual usually seeks medical advice. This discharge is commonly unilateral, more rarely bilateral, according to whether one antrum or both antra are affected. On looking into the nostril corresponding to the affected antrum, it is usual to see a collection of fluid pus on and about the concavity of the middle turbinated body; this, on being removed, rapidly reappears. From time to time this overflow pus from the antrum accumulates and escapes by the anterior nares, constituting the (anterior) nasal discharge already referred to as one of the most important signs of antral empyema. It is probably unnecessary to add that fluid pus in the nose may have other sources than

the antrum, such as the frontal sinuses and anterior ethmoidal cells, but I am of opinion that these are seldom or never affected without the participation of the antrum, and it is therefore requisite to thoroughly examine and if necessary treat the antrum in the first instance in all cases of persistent purulent discharge from the nose.

The point which it is more particularly intended to bring forward in this brief communication, is the possibility of chronic empyema of the maxillary antrum being present without anterior purulent nasal discharge, and in this connection I am desirous to submit the following propositions, with illustrative cases in their support :—

1. That chronic empyema of the maxillary antrum may be present without anterior, but with posterior nasal discharge; and

2. That it may be present for some time without anterior or posterior nasal discharge, but with the development of an odour perceptible by the sufferer only.

I.—Case of chronic empyema of the maxillary antrum without anterior, but with posterior, purulent nasal discharge.

A lady, aged forty-five years, was referred to me by Dr John Orr on 10th October 1899, on account of a persistent post-nasal discharge which had been present for several months. At the commencement of the complaint there had been a slight anterior nasal discharge which had soon completely disappeared. The upper teeth had been decayed and extracted several years previously. There was no discharge whatever by the anterior nares, and on anterior rhinoscopy no pus could be seen in the nose. On posterior rhinoscopy some fluid pus was detected in the posterior nares and naso-pharynx, and on more minute examination, it was found that this pus issued from the left nostril. Trans-illumination indicated a distinct darkening of the left cheek as compared with the right. On the 13th October an exploratory puncture was made, and fœtid pus was found in the left antrum. Under the usual treatment the patient made a rapid recovery, with total disappearance of the post-nasal discharge.

About the time that this case was under observation a somewhat similar one was reported very fully by Dr Brady, N.S.W. ("Chronic muco-purulent catarrh of the Antrum of Highmore simulating Post-Nasal Catarrh," *Journal of Laryngology*, vol. xiv., p. 565). In the case of this patient, a gentleman aged twenty-seven years, the trouble had followed on an attack of influenza three years previously, and had been

diagnosed as simple post-nasal catarrh by several specialists and surgeons in London and elsewhere. The trans-illumination test, as in the case now recorded, was correct; puncture of the antrum showed the presence of pus.

It is difficult to say why the overflow pus from an antral empyema should now and again be found to discharge posteriorly, but probably the internal configuration of the upper reaches of the nose has more to do with its causation than anything else. It is a moot point whether some of the examples of so-called post-nasal catarrh in its moist forms may not be of antral origin; hence, probably, their marked obstinacy to the usual methods of treatment. At anyrate enough is now known to justify a careful examination of the antrum in all cases where an anterior or posterior purulent nasal discharge is present.

II.—Case of chronic empyema of the maxillary antrum without (in the first instance) anterior or posterior nasal discharge, but with the development of a peculiar unilateral odour (parosmia).

Mr P. came to me on the 15th November 1898, on account of the presence of a disagreeable odour localised by him in the left nostril. This odour had been present for about two months, and was of a somewhat persistent nature. There was not, and had not been, any discharge from the nose anteriorly or posteriorly. The odour was not perceptible by the observer. Teeth decayed. On examination by anterior and posterior rhinoscopy, no pus or discharge of any kind could be seen. He was seen on three subsequent occasions with the same result. On the 13th February 1899 a very little pus was detected in the nose for the first time; it occupied the anterior portion of the left middle turbinated body. Trans-illumination showed slight opacity of the left cheek, etc., and an exploratory puncture of the left maxillary antrum revealed the presence of pus in the cavity. (Owing to a sudden call to return to his home in Natal, I have been unable to follow up the history of this case.)

While parosmia may be a symptom of grave disease of the nervous system, it appears to be more commonly of a functional nature, and indicative of such troubles as are described as hysterical. As a symptom of latent empyema of the maxillary antrum it has not hitherto obtained recognition, and its frequency and value in this respect cannot therefore be accurately estimated. In endeavouring to determine its presence, care must, of course, be taken not to confound it with such diseases of the nasal cavities as ozæna, in which, on account of the de-

struction of the terminals of the olfactory nerves, the odour is not perceived by the patient, and in judging of its value as a symptom I would suggest that it appears to be of some importance to determine whether it is *unilateral* or *bilateral*. Unilateral anæmia is more likely than the bilateral variety to have a local (organic) cause in the brain, or, as in the case now recorded, in the nasal regions. The only instance I can find of the employment of the term "unilateral parosmia" is in the record of a case quoted by Morell Mackenzie ("Diseases of the Throat and Nose," vol. ii., p. 472), in which on *post-mortem* examination a glioma pressing upon and effacing the left olfactory lobe was found.

But the point I wish to emphasise is, that the condition of the maxillary antrum ought not to be overlooked in cases of parosmia, more especially when the complaint is unilateral.

ON THE DISTURBANCES OF THE SYMPATHETIC SYSTEM IN LOBAR PNEUMONIA[1]

By J. EASON, M.B., C.M., M.R.C.P.Ed.,
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IN a communication to the Royal Medical Society, Edinburgh, in February 1899, I drew attention to the condition of the pupils in lobar pneumonia, and stated that there was an absence from the literature of pneumonia of any reference to it, at least so far as I had been able to discover. I have recently made a further examination of various works on medicine including Osler's, Fagge's, Taylor's, and Clifford Allbutt's "System of Medicine," and the more special writings of Powell (1), Fowler (2), and Sturges (3), without finding therein any reference whatever to the subject. I was, moreover, unable to find any allusion to certain other conditions in lobar pneumonia which I purpose considering in this paper, all of which would seem to depend on disturbance of the sympathetic nervous system. These are flushing, pallor, proptosis of the eyeball and the cerebral symptoms so often associated with apical pneumonias.

I shall first describe the observations made regarding the condition of the pupils in lobar pneumonia, and while the

[1] Read before the Royal Medical Society, Edinburgh.

number of cases, viz. 54, is by no means very large for statistical purposes, yet it seems probable that the figures which are to be given below, and are so striking, indicate some points which we may reasonably anticipate to be borne out by further observations. For the most part the cases were observed in Leith Hospital and the Children's Hospital, Edinburgh, with a few others from the Royal Infirmary, Edinburgh, and in private practice. Altogether 48 are recorded in hospital ward journals and 45 are consecutive cases. Now I find that of the total 54 cases a very large proportion had *inequality* of the pupils. Thus—total cases of lobar pneumonia, 54; with unequal pupils, 24; with equal pupils, 30. The percentage of cases with unequal pupils is thus 44 per cent., which is certainly very high. Further statistics may show that this proportion is too great, but the figures given indicate that a very large proportion of cases seem to have unequal pupils.

Examination of the 24 cases having unequal pupils appears to show that the inequality is owing to the occurrence of unusual *dilatation* of one pupil. It would appear that the large pupil is in some way determined by the side on which pneumonia is. Thus, of the 24 cases with unequal pupils, 21 had the large pupil on the side on which the signs of pneumonia were detected. There were, however, three cases in which the contrary obtained. As to these, I am unable, meantime, to give any satisfactory explanation or to say whether there were other disturbing influences at work in any of them, apart altogether from the pneumonia. It may be asked, if pneumonia causes inequality of the pupils why does it not always do so? Much light is thrown on this question when an analysis of the 54 cases is made with especial reference to the site of the pneumonia.

Thus, of the total cases of unequal pupils (24), there were (1) apical, 15; (2) near apex, 3; (3) middle, extending down or up, 4; (4) basal, 2. On the other hand, of the total cases with equal pupils (30), there were (1) apical (all children), 4; (2) basal, 26. The classification of cases into apical, basal, etc., must necessarily be to some extent arbitrary, but the above tables indicate as accurately as possible the site of the physical signs in each case as described in the case records. The figures would certainly seem to show that the *absence or*

presence of inequality of the pupils depends on the part of the lung affected. Thus while in 15 apical pneumonia cases there were unequal pupils, and 7 other cases presenting signs of pneumonia in the middle and upper part of the lung showed also unequal pupils, only two basal cases (*i.e.* with physical signs of pneumonia not above the middle of the scapula) had inequality, while 26 basal cases had equal pupils. It is significant also that the 4 apical cases having equal pupils were all in children, while no apical case in adults had equal pupils. These facts seem to show that inequality of the pupils may be expected in pneumonia affecting the *apex*, while in cases with physical signs confined to the *bases* the probability is that the pupils may be equal. Of the 54 cases there have been only 4 of double pneumonia, and it was observed in all of these cases that the pupil was larger on that side on which the lung showed the physical signs of pneumonia nearest to the apex. It is possible that fuller statistics may bear out what the uniformity of these cases indicates. In the class of cases where, from the state of exhaustion of the patient, it is considered undesirable to examine the posterior part of the thorax daily, constant observation of the pupils from day to day may give an indication that the previously healthy lung has become involved. Thus I have full notes of two cases in which this has actually occurred.

The time of onset and of disappearance of pupil inequality would seem to be very variable. Thus, when the physical signs of pneumonia indicate that the apex is involved, the pupil inequality appears either to anticipate these signs or to appear simultaneously with them; but when physical signs indicate that pneumonia involves the lung at some distance from the apex, then inequality may not be seen for some days after the physical signs are fully developed, and it has been detected for the first time as late as the fifth day after the crisis, although closely looked for throughout.

The duration of the inequality in apical cases appears to be greater than in other cases. It has been present eight days after the crisis. It may pass off and return. Is it possible that persistence of pupil inequality for a longer time after the crisis may indicate that resolution is not altogether satisfactory?

The condition of the pupils, above referred to, may, I suggest, be explained by reason of the irritation which the

pneumonia causes to that part of the sympathetic system which is known to act as the dilator of the pupil. These fibres are said to pass from the first, second, and third dorsal nerves to the inferior cervical ganglion, and in their course from the one to the other must be intimately related to the lung at its uppermost part. This would explain why it is that apical pneumonias appear specially liable to cause inequality of the pupils.

As already stated, there appear to be other indications that the sympathetic in this region is irritated in lobar pneumonia, as, for instance, the flushing and pallor of the face. The former, as is a matter of common observation, occurs on the cheek usually of the *same* side as that on which the pneumonia is situated. Is it possible to explain this condition on physiological grounds? Now, it is maintained by physiologists that the cervical sympathetic contains vaso-dilator fibres for the face, and that these fibres course through the thorax from the first to the fifth dorsal nerves towards the inferior cervical ganglion. Stimulation of these fibres produces flushing of the face, and it would seem that in pneumonia the flushing is thus caused. But flushing does not always occur. On the contrary there may be a very marked pallor of the cheek accompanied or unaccompanied by dilatation of the pupil on the same side as the pneumonia. I have not found any record of this condition in the books, but I possess notes of an apical case occurring in Leith Hospital, and having simultaneously inequality of the pupils and pallor of the face. Another case is also known to me having the latter condition without the former. It seems probable that the physiological explanation of this may be an irritation of the vaso-constrictor fibres for the face passing from the second, third, and fourth dorsal nerves to the inferior cervical ganglion. It is interesting also to note that physiologists teach that of the two sets of fibres, vaso-dilator and vaso-constrictor, the former are much more easily stimulated, and the effect of the stimulation is also much more durable. In this fact is it not possible to find an explanation for the more frequent occurrence of flushing compared to that of pallor?

I have also observed clinically another condition in lobar pneumonia, and of which I have failed to find any previous record. Thus, in two cases, I have noticed distinct though not very great protrusion of the eyeball, with widening of the

palpebral fissure and dilatation of the pupil on the same side on which the physical signs of lobar pneumonia were detected. The same suggestion which was brought forward to explain the flushing, pallor, and the dilatation of the pupil, may again be used regarding the protrusion of the eyeball, as the fibres in the sympathetic which act on Müller's muscle pass from the upper four or five dorsal nerves to the sympathetic in the neck.

Another point worthy of attention is the statement made by Donders (4) and others (though denied by Leonard Hill (4), that these same fibres are also vaso-motor for the brain. The significance of this depends on the characteristic cerebral symptoms of apical pneumonias, for the occurrence of which no satisfactory reason has, so far as I am aware, been assigned. In any case I do not think it is denied by any that either directly or indirectly the vaso-motor mechanism controls the cerebral circulation, and it appears that in lobar pneumonia the vaso-motor apparatus is more or less profoundly affected.

The part of the sympathetic already so frequently spoken of possesses one other important function, viz., it supplies the accelerator fibres to the cardiac plexus. Is it possible, in apical pneumonias or other cases of pneumonia in which there are evidences of what have here been considered disturbances of the sympathetic, that we may sometimes expect to find the characteristic respiratory-cardiac ratio of pneumonia disturbed by the occurrence of irritation to these accelerator fibres? This question is prompted by theoretical grounds only, as no clinical observations were made regarding it in any of the fifty-four cases.

For permission to examine the ward journals of Leith Hospital during the period subsequent to my term as Resident Physician there, I have much pleasure in acknowledging the kindness of Dr Wm. Elder and Dr Langwill, physicians to the hospital. For help in many ways I am also indebted to Dr Langwill, in whose wards the observations above recorded were begun. I thank Drs John Thomson, Noel Paton, Herring and Callander for information given regarding references.

LITERATURE.—(1) Powell's *Phthisis and other Diseases of the Lung*, 4th ed., 1893. (2) Fowler and Godlee, *The Diseases of the Lungs*, 1898. (3) Sturges and Coupland's *Pneumonia*, 2nd ed., 1890. (4) Leonard Hill, *On the Physiology and Pathology of the Cerebral Circulation*, page 45, 1896.

Meetings of Societies

Glasgow Medico-Chirurgical Society

AT a meeting on March 16th.

Dr M'Call Anderson showed (*a*) a case of chorea, with special reference to treatment. The patient was a girl, aged nine years, who had suffered from chorea for two years. She had improved under treatment in the children's hospital, but had relapsed. At the time she came under Dr Anderson's treatment she had suffered seriously for months. She could not walk without assistance, or even stand without support, had difficulty in speaking, and was unable to feed herself.

Chorea was troublesome as a rule to treat. In this case the results had been very satisfactory. The plan of giving large doses of antipyrin had been adopted—beginning at five grains thrice daily, and gradually increasing the dose, till as much as 135 grains were given in the day. The only trouble with this large dose was occasional vomiting. The treatment had been commenced on January 22nd, and continued to the date of meeting, when the child was shown to the Society, with only a very slight trace of chorea present. She walked well, stood very fairly still, and clearly her condition was in marked contrast to that in which she had been admitted to Dr M'Call Anderson's wards. Dr M'Call Anderson said that small doses of antipyrin were useless in chorea, and the treatment could only be said to be properly begun when quite large doses were being taken. He believed that in the treatment of disease by medicines, much greater benefit would be achieved, if we were able to push the doses to a much greater extent than we usually did. He had had many cases in which large doses of antipyrin had produced very good effects.

(*b*) A case of hemiplegia, with special reference to diagnosis and treatment. The patient was a rivetter, aged forty-five who was admitted, under Dr M'Call Anderson, suffering from left hemiplegia of four days' duration. There was the usual partial paralysis of the face, and *total* loss of power in the left arm and leg. The patient had awakened one morning to find himself unable to rise from bed, as the paralysis had developed during sleep. He had been previously healthy, although given to excess in alcohol. There being no evidence of heart disease, embolism was excluded. Nor was there evidence of hæmorrhage, the state of the patient's arteries and kidneys being against this view. As there was slight enlargement of the glands in the groin, and of the epitrochlear glands in the arms, and a history

of gonorrhœa, though not definitely of syphilis, and a slight coppery tint of an old scar in the leg, Dr M'Call Anderson, though perceiving that no one of these signs was alone conclusive, considered that the sum of evidence pointed to the cerebral lesion being syphilitic in nature. Treatment by large doses of iodide of potassium was commenced, with 10 grains thrice daily at first, at December 20th, while by January 4th he was taking 120 grains thrice daily. So far as iodism was concerned these large doses were safer than small ones.

The patient was shown to the meeting perfectly recovered in all respects. The lesion was probably a gumma pressing on one of the central blood-vessels and interfering with blood supply, and not a condition of thrombosis from disease of the intima of the blood-vessel.

Dr Wallace Anderson had lately been afraid of giving large doses of antipyrin. He had seen fainting follow a dose of 20 grains, but had not seen further bad results from such doses. With such large doses as Dr M'Call Anderson had administered, he was unfamiliar. In the case of hemiplegia he agreed with the diagnosis of syphilis, and was again struck with the very large doses of iodide used. He had seen iodism follow a dose of one grain.

Dr Howie asked if anything was combined with the iodide, such as cinchona, to diminish the chance of iodism, and was answered in the negative. Water only was used as a diluent.

Dr Grant asked if Dr M'Call Anderson had any special preference for antipyrin, instead of any of the other coal-tar derivations, and was answered in the negative. Dr Anderson believing that other drugs of the same class might do as well.

Dr Adamson asked, with a view to the question of diagnosis, how soon improvement had taken place after admission to hospital. Dr M'Call Anderson stated, generally, that in about ten days time the patient was sitting up in bed, reading the papers, and with very good movement in the affected limbs. Dr Adamson thought this very rapid improvement suggested the possibility of embolism as the cause of the lesion, but he understood Dr Anderson had considered that and excluded it.

Dr M'Call Anderson, in a brief general reply, considered that there was too great a tendency to confine dosage within certain limits. There were remarkable idiosyncrasies in the matter of dosage, as was well known.

(2) Drs Walker Downie and R. Kennedy showed (*a*) a patient suffering from stricture of the œsophagus, in whom gastrostomy (Frank's operation) was performed nine months ago. In this patient there was detected a hard stricture about the level of the second tracheal cartilage. There was also left

abductor paralysis. Traumatism, malignant and tubercular disease were excluded from the diagnosis, and it was ultimately considered to be syphilitic in nature. Iodide of potassium was prescribed, and the regular passage of bougies. She improved and ceased from attending for treatment, returning, when she was much worse in all respects, with serious loss of weight.

Dr Kennedy found the case suitable for gastrostomy, which he performed. The details of this operation as performed on the patient were minutely described to the Society, and the patient shown, with the tube in position. She had been fed by nutrient enemata till the fourth day, when the opening into the stomach had been completed, and since then large amounts of nourishment had been introduced satisfactorily into the stomach and digested. The woman, when seen, had a healthy appearance, not at all suggestive of serious disease. The condition of the throat was unfortunately much worse. There was bi-lateral abductor paralysis, and considerable breathlessness on exertion. There was absolutely no power to swallow anything by the mouth. There was a tendency to the formation of a ventral hernia at the site of operation.

(*b*) Drs Walker Downie and R. Kennedy also showed the parts from a case of extensor primary tubercular ulceration of the œsophagus, in which gastrostomy (Witzel's operation) had been performed.

In that case there was found a stricture about the level of the first tracheal cartilage. The parts were soft and swollen, and bled readily on the passage of the bougie. There had been first difficulty in swallowing, and then pain, and finally impaction of food in the gullet. The lungs and mediastinum were found to be healthy, also the vocal cords. The pharynx was congested. There was evidence in the neck of old extensor tubercular disease. Cancer, traumatism, syphilis, and spinal disease were excluded, and a diagnosis of tubercular ulceration and stricture of the gullet was made. This proved to be correct, and the specimen (the patient having died some weeks after the operation) was shown to the Society.

Dr Kennedy described minutely the steps of Witzel's operation. It was very successful, and the severe pain in the throat and neck, from which the patient had suffered for long, was relieved forty-eight hours after. The palliation, which was all that was hoped for, was thus achieved. The patient developed pyuria, a tubercular abscess in the neck, perforating the trachea, pulmonary disease, and died in some weeks.

Dr Kennedy contrasted Frank's and Witzel's operations, stating the advantages and disadvantages of each, and the reasons of his choice. In conclusion he came to the opinion

that Witzel's operation should be preferred to Frank's, both in cases where the patient was expected to survive for a long period, and in those in which only a short time would elapse before death.

Dr Ferguson had made the *post-mortem* examination and had found evidence of tubercular infiltration of the floor of the œsophagal ulcer, and of extensor old tubercular disease in the glands of the neck. The ulcer extended for three inches in the axis of the gullet. An abscess had formed between the gullet and the trachea, and the necrosis of a tracheal ring had occurred, with perforation. The lungs showed no definite tubercular lesion, but many intensely septic foci—metastatic abscesses. It was difficult to account for the pus in the urine. The kidneys were not tubercular. Possibly the condition of the alimentary tract had infected the mucous membrane of the urethra and bladder, apart from a blood infection.

Dr M'Call Anderson, in reference to the first case, spoke of the symptoms of a mediastinal tumour, and suggested the possibility of such being present, but Dr Downie had no difficulty in convincing the Society that the diagnosis given by him was substantially correct.

(3) Dr James W. Allan read a paper on "Lectureships on Trade Diseases." This subject was treated in a very interesting manner. Dr Allan pointed out that we were behind our continental neighbours in this matter. He had corresponded with authorities on the subject in Germany, France, and America, and he hoped to see more attention paid to it in this country. The paper was warmly received by the Society, and a wish was expressed for its early publication.

Dr Scott, in thanking Dr Allan for his paper, said he had had much experience in dangerous trades. He would go farther than Dr Allan, and thought that a medical man should be appointed with responsibility for the lives of the men, not only to the employer, but to the Government. Medical men had a duty towards the industrial classes in these matters, and they were performing it. All the amendments to the Factory Acts had been brought about by medical men.

Glasgow Obstetrical and Gynæcological Society

AT a meeting on March 14th.

1. Dr Carstairs C. Douglas read a short paper entitled "Some Observations regarding the Sterilization of Milk in infant feeding." Dr Douglas, in the course of his remarks, said that he looked with suspicion on a milk so prepared that it would keep

sterile for a long period In such case its antiscorbutic properties were minimised. In speaking of the pasteurising of milk on a large scale, as he had seen it carried out in dairies in Glasgow, he could not say that the process was satisfactory. There were abundant opportunities for contamination of the milk after it had been heated, as it was freely exposed to the air and allowed to cool naturally, which it did but slowly, owing to the high specific heat of the milk.

Dr Henderson referred to the imperfections in the method used by dairies, as well as the additional risks in the modes of distribution.

2. Dr J. M. Munro Kerr read "Notes of case of Infantile Uterus." The patient was twenty-three years of age, unmarried, and complaining of great pain, lasting two days, every fourth week. No menses present. Bimanually the uterus was found to be very small, and the sound passed only 1½ inches. There was a swelling on the right side of the uterus, thought to be ovarian. Dr Kerr referred the dysmenorrhœa to the undeveloped condition of the uterus—but the pain resisting treatment, he, after six months, opened the abdomen, and removed an enlarged cystic right ovary and a small left ovary. The uterus was seen at the operation to be typically infantile. An excellent recovery was made, and the patient was now perfectly well and free from pain. On examination of the ovaries there was found proof that ovulation had taken place, although there had never been any menstrual discharge at any time. Some authorities, Winckel and others, say ovulation does not occur in cases of infantile uterus, but in this case there was no doubt at all that it did.

Dr Miller congratulated Dr Kerr as having brought forward a very interesting case, and considered it important that all such cases should be put on record.

Dr Balfour Marshall narrated the details of four somewhat similar cases, all occurring, however, in married women, and also mentioned a case of super-involution following severe post-partum hæmorrhage as a contrast.

Dr Edgar mentioned another similar case, a small uterus, dyspareunia, and amenorrhœa, with an ovarian cyst the size of an apple pushing the uterus over to the left side. The condition of infantile uterus was not a satisfactory one to treat, though when menstrual molimina were present treatment should be tried. He had seen the galvano-stem pessary tried in these cases, but with very uncertain and temporary results.

Dr Howie had recently seen a very similar case.

Dr Kerr, in reply, said that care had to be taken to distinguish merely small badly developed uteri from the true

condition of infantile uterus. In his case he had actually seen the uterus at the operation, so there was no doubt about it.

3. Dr Edgar read "Notes of four cases simulating Extra-uterine Pregnancy, with remarks on the diagnosis." He stated that cases of extra-uterine pregnancy were comparatively frequent. In hospital work during the past three and a half years he had seen, out of four hundred and fifty cases under his care, twenty-six cases of ectopic gestation, and in that period he had also seen five cases in private practice. Thus the disease could not be regarded as rare, and it was most important to be on the watch for it. A fairly typical case was then described, noting the cramp-like abdominal pains and collapse, with abdominal distension, and the uterine hæmorrhage which set in usually and tended to persist.

With regard to differential diagnosis, it was well to know that many of the symptoms might be absent, and a seemingly typical case might really be something quite different. Perhaps one of the commonest errors was to consider such a case one of ordinary uterine abortion. This was easier to do, apparently, than possibly it seemed to some. Of course a careful local examination usually made matters clear. The cases most difficult to diagnose were—(1) those of pelvic hæmatocele due to other causes; (2) conditions of pelvic cellulitis and exudation.

Dr Edgar then narrated a case, and showed the specimen, of pelvic hæmatocele due to a ruptured hæmatoma of the ovary, and detailed also four cases of pyosalpinx, in which there were sudden abdominal pains and collapse associated with history of amenorrhœa, and mentioned that Martin had put on record five cases of apparently typical extra-uterine pregnancy, which were in reality pyosalpinx with exudative inflammation.

Dr Edgar then described (and showed the specimens) cases of tubercular disease of the uterus and tubercular pyosalpinx, and of suppurating intraligamentous ovarian cyst, both simulating extra-uterine pregnancy, and bringing out some interesting points in the diagnosis, and the importance of seeking a careful history of these cases, with reference to former feverish symptoms, rigors, etc. This, especially with hospital patients, was often very difficult to obtain.

Dr Miller thanked Dr Edgar for his most interesting paper. As showing the difficulty in arriving at a diagnosis from general symptoms of pain, collapse, even pelvic pain, he narrated the chief features of a case seen by him and Dr Parry, in which the symptoms were very much those described by Dr Edgar, or typical of a ruptured extra-uterine gestation, while the condition was really one of perforated gastric ulcer.

Dr Munro Kerr thought with Dr Edgar that the commonest

error was to suppose that ordinary abortion existed when really extra-uterine gestation was present. This should not occur with ordinary care and examination of cases. He certainly thought that mistakes might easily arise in connection with inflammatory pelvic conditions. It was possible also to mistake an extra-uterine gestation for a retrovertal gravid uterus, and cases of this kind had been recorded by Barbour, Croom, Cullingworth, and others. He had met with a case of this nature himself. Unlike some of the other conditions which might give rise to difficulty or error in diagnosis, this particular condition was a serious one, and exact diagnosis was of vital importance. In these cases it had occurred that efforts to replace the supposed retrovertal gravid uterus had resulted in rupture of the extra-uterine sac, with very serious results. There was no doubt also that suppurating tubes gave rise to conditions which might easily be taken for extra-uterine gestation.

Dr Edgar, in reply, stated that so far as he had observed the site of pain in these cases, it was usually pelvic. The next most frequent seat for the pain to be referred to was the umbilicus. He drew attention to the importance of investigating both the situations of the subjective pains and the objective tenderness to pressure.

Obstetrical Society of Edinburgh

THE fifth ordinary meeting of the Edinburgh Obstetrical Society was held on the 14th March. Dr Milne Murray, President, in the chair.

Dr Foulis exhibited two interesting specimens under the microscope in order to show that the umbilical cord does not have a covering of amnion.

Dr J. W. Ballantyne described two schemes which he had drawn up in order to note points in the chronology of antenatal life. Each is based on the space for time method introduced by Mr Jonathan Hutchinson. In the different spaces in each chart one may note the various events, normal or abnormal, which may occur during pregnancy.

Dr J. Halliday Croom read two papers on (1) *Chylous Ascites*, with report of a case. He stated that obstruction to the thoracic duct may give rise to no symptoms during life; in other cases, on the other hand, very definite signs may be present, and this is specially marked when the abdominal cavity is involved in the pathological process. Sixty-three cases of chylous ascites had been recorded up to the present. The case reported occurred in a woman, aged thirty-nine, who had given birth to six children, of whom the youngest was three and a half

years. Two months previous to her admission to the hospital she had noticed a gradual enlargement of the abdomen, together with progressive weakness and emaciation. Hard masses were felt about the umbilical region, and the diagnosis made was that of cancer of the omentum with ascites. On tapping the abdomen 380 oz. of fluid like pus but more mobile were removed. It had a sweet milky odour and was slightly opalescent. The fluid was examined by Dr Milroy of the Physiological Laboratory and was found to contain 2 per cent. of fat, and was in fact a fine emulsion of fat showing fatty leucocytes and granular debris. A movable tumour was detected in the region of the gall-bladder and another in the umbilical region. The woman became more emaciated and exhausted and ultimately died. The examination after death showed that there were numerous minute subpleural cancerous nodules and commencing peritonitis in the abdomen. A large fungating gelatinous mass of cancer was growing from the anterior abdominal wall, and the liver contained also a small nodule. The small intestine was greatly infiltrated by the cancerous growths.

(2) *An interesting case of Double Ovariotomy.* The patient was a girl, aged sixteen and a half years, who had never menstruated. The abdomen was greatly distended and the patient much emaciated. The right plural sac contained 144 oz. of fluid, and what was supposed to be a malignant ovarian tumour was present in the left lower part of the abdomen. The abdomen was tapped once or twice a week for some months, but as the patient did not get progressively weaker, it was then thought that it might not be cancerous and might be removed. On opening the abdomen a large ovarian tumour of about 30 lbs. weight was present and was removed. The right ovary appeared healthy and was left. Menstruation soon commenced after this operation. The patient was readmitted more than a year subsequently for the removal of a second cystic ovarian tumour of the right ovary. Dr Croom remarked that when there was any doubt as regards the diagnosis, an exploratory incision should be made.

Remarks on these papers were made by several members.

Dr Jardine, of Glasgow, gave a further contribution on the "Treatment of Eclampsia by Saline Infusion." The author related his experience in five new cases treated since the New Year by saline infusion (one drachm of acetate and chloride of sodium to each pint). Together with the infusion, use was made of chloral and bromide and veratrum viride hypodermically. In all, Dr Jardine has treated in this way twenty-two cases of eclampsia, and the mortality was only four. There is no risk in the proceeding.

Dr Haultain read a paper on "Pathological Amenorrhœa

from other than Constitutional Causes, with illustrative cases." As causes he noted—(1) atresia vaginalis; (2) atresia uterina; (3) absence of ovaries; (4) undeveloped uterus; (5) central nervous system in fault, as where both ovaries and uterus appear normal in size; (6) acquired amenorrhœa due to some nervous change, as from fear, over-work, etc.; (7) prolonged cessation from anæmia may lead to a permanent amenorrhœa.

Illustrative cases were narrated as regards treatment; in acquired amenorrhœa much may be done, but uniform success cannot be hoped for. According to the size of the uterus, so the treatment must be regulated. The author has found that in many cases the constant current, using the negative pole in the uterus, and with from 40 to 60 milliampères of current, has proved of great benefit. He has also obtained excellent results from the employment of ovarian extract. In cases of premature climacteric, the flushings and headaches are often relieved by its use. The menstrual function may even be restored by its administration, though pregnancy need not be hoped for. Ovarian extract is of great value in cases of diminished menstruation with great obesity. Certain cases should not always be looked upon as incurable, and as resulting from an early menopause, or from superinvolution of the uterus.

Discussion on this paper was deferred until the next meeting of the Society.

Aberdeen Medico-Chirurgical Society

A MEETING of this Society was held on 1st March, Dr W. H. Williamson, President, in the chair.

The evening was devoted to a demonstration of *pathological* specimens.

Dr G. M. Duncan showed (1) myxo-sarcoma of femur, with secondary deposit in the right clavicle; (2) small round cell sarcoma of clavicle, with secondary deposit in the lung; (3) multiple adenomata of kidney, with slide; (4) slide from urine of a case of renal phthisis; (5) slides of blood from leucocythæmia and pernicious anæmia.

Dr Edmond showed specimens of tumours of the frontal lobe and of the cerebellum, with drawings and photographs.

Dr Gordon exhibited cases of (1) cancer of the hepatic flexure of the colon, and (2) tubercular larynx.

Dr A. W. Mackintosh showed (1) a case of traumatic cerebellar hæmorrhage; (2) a slide of cystin crystals; (3) sections of cords from cases of "railway spine" and acute myelitis.

Dr Marnoch showed sections of appendix vermiformis

varicose veins, sarcomata of parotid and submaxillary glands, retained testis and cystic ovary.

Dr Rose showed two specimens of sarcoma of the kidney, removed by operation.

Dr Stephenson showed (1) a fœtal monster, with skiagram; (2) a specimen of carcinoma uteri; (3) an epithelial cast from vagina, resulting from treatment with ichthyol.

Dr Usher exhibited jelly specimens and sections of tumours —(1) epibulbar, *e.g.* dermoid tumour, pigmented epithelioma; (2) intra-ocular, mainly sarcomatous; (3) orbital, *e.g.* sarcoma, cavernous, angioma, etc.

Dr Walker exhibited a medium suitable for the rapid growth of the diphtheritic bacillus.

Forfarshire Medical Association

AN additional meeting of this Association was held in the University College, Dundee, at 3.30 P.M., on Wednesday, 7th March, Dr Templeman, Vice-President, in the chair.

Mr Greig showed a patient, a man, after excision of the left upper jaw for sarcoma, and pointed out the slight deformity following such an operation.

Mr Greig also showed a boy with coxa-vara, pointing out the altered movements of the hip joint and the attitude in standing and lying which is exhibited by patients suffering from this affection.

Mr Greig also showed several specimens—

1. The upper jaw with tumour attached, removed from the former patient.

2. A femur removed by amputation for acute osteomyelitis.

3. Two specimens with loose bodies in the tunica vaginalis testis, removed after death from a case of lympho-sarcoma.

Professor C. R. Marshall spoke on the nitrites and allied substances, giving his classification of them, and illustrating by means of the blackboard and sphygmographic tracings the actions of the various classes. He explained how it could be shown by experiment that these substances only acted on the blood-vessels and not on the heart. He also pointed out the various steps that had been taken in order to find a substance with a similar action to nitroglycerine or glycerol nitrate, but which had a more prolonged or lasting action. The toxic effects of these substances was then shown to be due to their power of forming methæmoglobin in the blood, and that the sodium nitrate was in this more rapid than nitroglycerine.

The therapeutic uses of these substances was next touched on, in angina pectoris and in cardiac failure.

Professor Stalker in some remarks on the paper emphasised the beneficial effects of iodide of potassium in arterial sclerosis. The Chairman also commented on the paper, and Professor Marshall replied.

Dr George Whyte gave an account of a case of corrosive sublimate poisoning, which had resulted in recovery, partly due to vomiting being induced early and to its solubility in alcohol. The quantity taken was equal to 96 grs. The symptoms, severe vomiting and purging, were treated by 20 minims of tincture of opium every four hours to begin with. The patient resumed his occupation on the fifth day after taking the poison.

Professor Marshall and the Chairman made some remarks on the subject, and the difficulty in knowing the exact amount taken.

Professor Stalker gave notes of a case of sub-acute endocarditis which he had had under his care. The case showed some difficulty in the diagnosis. There was a persistent temperature, some cough, and later a mitral systolic was made out, but for a time it resembled a case of pulmonary tuberculosis. There was no rheumatic history, and the case did not present the appearance of a cardiac case.

Professor L. R. Sutherland showed the heart from this case which had vegetations in the mitral cusps. Several of the chordeæ tendineæ had given way, causing the valve to flap during the heart's action and infect the endocardium in the auricle.

He pointed out that the orifice though in life must have been narrowed, still admitted two fingers.

He considered that the separation of chordeæ tendineæ was not such a rare affection as was generally made out.

Dr Foggie emphasised the importance of watching the temperature in such cases.

New Books and New Editions

A System of Medicine. By Many Writers. Edited by THOMAS CLIFFORD ALLBUTT, M.D., F.R.C.P., F.R.S., Regius Professor of Physic in the University of Cambridge. Vol. VIII., pp. 998. London: Macmillan & Co., Ltd. Price 25s. net.

THIS volume opens with the concluding sections on the nervous system. The first chapter on Craft Palsies is by Dr Vivian Poore who deals with the subject in a philosophical, and at the same time in a thoroughly practical fashion. Dr Risien Russell discusses Tetany both in the adult and in the child at great

length, and shows an extensive acquaintance with the literature on the subject. He leans to the view that the condition is due to an auto-intoxication acting upon spinal neurons. His clinical descriptions and treatment are alike admirable. Sir William Gowers' short chapter on that distressing malady, Paralysis Agitans, is characterised by the author's crispness and clearness of style. Our experience, which is doubtless more limited than Sir William's, would give more cases under forty years of age; and, while his view of the probable morbid change demands respectful consideration, we are not satisfied that a premature senility in the nutrition of cortical neurons has much to support it. Treatment is as futile in his hands as in those possessed of less cunning. In the short chapter on Migraine by Dr James Mackenzie, the writer does not give us the impression of having been as successful in his treatment as many others have been since the introduction of phenacetin and antipyrin. The chapter on Hysteria by Dr Ormerod is estimable; and the short chapter by Dr Patrick Manson on Latah will be of almost unique interest to many readers. Neurasthenia, by Dr Clifford Allbutt, shows all the lightness of touch and clinical acumen which, as has been repeatedly noted in other chapters from his pen, characterise the writings of the editor of this *System of Medicine*. The subject of Traumatic Neurasthenia, which is often of great legal difficulty, is dealt with by Mr Victor Horsley.

The next section in this volume is devoted to Mental Diseases, and the editor has had the assistance of Dr Savage in its preparation. The most interesting chapter here is that by Dr Mercier on Vice, Crime, and Insanity. It is full of close and powerful reasoning, and is well worthy of perusal by all who have any interest in the relations of the three conditions discussed. Useful chapters are contributed by Dr Clouston, Sir John Batty Tuke, Dr Urquhart, and others.

The concluding section of the volume is devoted to Diseases of the Skin. The chapters are all by well-known authorities, whose names guarantee accuracy of description, and large practical experience of the results of treatment. They call for no specialised notice.

An appendix on Malarial Fever by Dr Patrick Manson brings the subject down to date.

At the end of the volume is a general index to the whole *System of Medicine*, and will be of great assistance for general reference.

This volume concludes the series, and we offer our congratulations to Dr Clifford Allbutt on the completion of a work which easily takes the foremost place amongst works of the kind in the English language. Of all books on medicine it is the one it would be well for every practitioner to have beside him for constant reference and mental refreshment.

Encyclopædia Medica. Under the general editorship of CHALMERS WATSON, M.B. Vol. ii., Brachial Plexus to Digestion. Pp. 562. Edinburgh: William Green & Sons.

THE first important word in this volume is "brain," and under it we find described by different contributors the physiology, the diseases of blood-vessels, cerebral tumours, atrophy, etc., and intracranial surgery; then under C we find "cerebellum," as if it were not part of the brain, and had no relations to intracranial surgery. Under "chest, clinical examination of," there is an account of the physical examination of the lungs and the heart; then we find under "bronchitis" and "bronchiectasis" accounts of these two conditions. These are but examples of the extraordinary difficulty attending the production of a work of this kind unless it has been elaborately and carefully planned out from A to Z before a word of it was written, and we are bound to say it bears no such impress. The articles are lightly written and contain much useful and accurate information in a concise form. There is no room for discursiveness and there is thus a certain tone of dogmatism which may be unavoidable, but is not pleasant to the critical reader. We cannot attempt to criticise this volume in detail; it could only be done partially, and the selection of articles either for commendation or condemnation would be invidious and neither fair to those dealt with or to those omitted.

Bacteriology in Medicine and Surgery. By W. H. PARK, M.D., Professor of Bacteriology, Bellevue Medical College, New York. Pp. 693. London: Henry Kimpton. 1900. Price 15s. net.

HERE we have a systematic account of all the Pathogenic Bacteria, with useful chapters on technique, immunity, disinfection, etc. The bacteriological diagnosis of diphtheria and of cholera are very fully gone into; so also is the serum reaction for typhoid.

On the plague bacillus we have a right to expect much more complete information, considering the vast amount of work recently done on that subject. We are surprised to learn that "no bacteria have been found in small-pox which seem to have any relation to the disease except as secondary infections," and that "the same is true of vaccinia." The admirable work of Klein and of Copeman is not even referred to.

But in spite of a few short-comings such as these, we can recommend the book in general to the student, practitioner, and health officer as a safe and well written guide to practical work.

Practice of Medicine. A Manual for Students and Practitioners. By GEORGE E. MALSBARY, M.D. Pp. 400. London: Henry Kimpton. 1900. Price 7s. 6d.

THIS book, by the Assistant to the Professor of Medicine in the Medical College of Ohio, is a brief epitome of the Practice of Medicine. The information is accurate and up-to-date, but is put too tersely for those who are without some previous knowledge of the subject; students preparing for examination will find it a handy resumé.

Editorial Notes and News

The Chair of Medicine in Edinburgh.

In our last issue we referred to four Edinburgh candidates for this chair. No other local candidates have so far been announced. An additional candidate is, however, understood to be in the field in the person of Dr Osler, of Johns Hopkins University, Baltimore. Dr Osler has frequently visited this country, and been accorded the hospitality and courtesy always extended to prominent strangers. He is known as an excellent clinician; as an industrious worker, who has recorded much of his clinical experience; and as the author of a text-book on medicine, which, like many recent American books, has found a market in this country. It is interesting that he should be willing to leave America to come to Scotland; but we do not regard him professionally or otherwise superior to several of the Edinburgh candidates. We unhesitatingly take up the position that an Edinburgh candidate is entitled to promotion at this time, and that in the best interests of the University a man with a record of long and successful teaching in a large school, such as all the Edinburgh candidates have had, ought to be appointed. Johns Hopkins University had its first batch of Medical Graduates in 1896, and

the number was 15. At the end of the academic year 1898 there were only 167 students enrolled as candidates for the degree of Doctor of Medicine. Dr Osler and his assistants do all the clinical teaching in medicine in the Johns Hopkins Hospital, on much the same lines apparently as it is done in Edinburgh, but we do not find in the reports any reference to the systematic teaching of medicine such as is necessary in Edinburgh.

Retirement of Professor Sir William Gairdner.

Sir William Gairdner has intimated his resignation of the Professorship of Medicine in the Glasgow University. We regret to see that he has stated as his reason for doing so, a consciousness of physical inability. It is understood that he is to reside in Edinburgh, where he will be welcomed by the profession. We hope that he may yet enjoy many years of comfort and of usefulness although their activities are to be curtailed. Already there is talk of his probable successor. The chair is in the gift of the Crown. The great difference between Edinburgh and the other Scottish Schools is the existence in the former of a large body of Lecturers in the extra-mural school, all of whom are lecturing and teaching as fully as the occupant of the chair in the University. This, especially in all clinical departments, provides Edinburgh with a choice of local men which the other Scottish Schools do not enjoy.

Appointment to the Chair of Systematic Surgery in Glasgow.

Dr Hector Cameron of Glasgow has been appointed Dr George Buchanan's successor in the chair of Systematic Surgery in Glasgow University. Dr Cameron is the present President of the Faculty of Physicians and Surgeons; he has been for many years a member of the Surgical Staff of the Western Infirmary, and a Lecturer on Clinical Surgery. He is a member of the Glasgow University Court, and he represents the Faculty on the General Medical Council. He is a popular and respected member of the profession in the West of Scotland, and has a good reputation as a surgeon. He has contributed some papers on surgical subjects to the medical journals.

Edinburgh and East of Scotland South African Hospital.

The energetic Lord Provost of Edinburgh has been most prompt and successful in carrying out his scheme to have an Edinburgh and East of Scotland Hospital for South Africa.

The money, some £20,000, was subscribed in a few weeks, and the entire staff left Edinburgh on the 22nd day of March and were accorded a befittingly enthusiastic send-off. The chief executive officer is Major Sir James Clarke, Bart., R.A.M.C. The civil staff consists of Professor Chiene as head surgeon, Mr David Wallace, second in command, and Dr F. D. Boyd as physician. The four assistant surgeons are Mr A. H. Watt, Dr C. M. Cooper, Dr G. L. Chiene, and Dr James Miller. The hospital consists of 100 beds, and is to be a stationary hospital on the lines of communication. The Lord Provost is to be warmly congratulated on the *éclat* with which he has carried this scheme through. We hope to be able in due time to give some account of the operations at the hospital to the readers of *The Scottish Medical and Surgical Journal.*

The Scottish National Red Cross Hospital for South Africa.

This scheme is also progressing favourably, but is on a larger scale than that which has already been sent off from the East of Scotland. The first section of 100 beds will be ready for dispatch about the middle of April, if transport can be provided for it. Mr Henry Clarke, Surgeon to the Royal Infirmary, Glasgow, has been appointed surgeon in charge, with Mr Luke of the same Infirmary as second surgeon.

International Congress of Professional Medicine and Medical Deontology, Paris, 1900.

The Committee of the International Congress of Professional Medicine and Medical Deontology, which will be held in Paris from the 23rd to the 28th July, desires to bring the following facts to the notice of the Medical profession:—

A reduction of 50% on the French Railways and of 30% on the steamers of the "Compagnie Transatlantique" will be granted to all persons who shall have paid their subscriptions before the 20th June 1900. The necessary vouchers for obtaining this reduction will be sent out at the same time as the card of membership of the Congress. The railway tickets will be valid for one month—from 20th July to 20th August.

As regards apartments in Paris, the Committee has endeavoured to make special arrangements with different agencies. Circulars giving full details are now in the possession of the presidents and secretaries of the French Committee of Patronage and of the various foreign national committees, to whom Members of Congress are recommended to apply for information on this point.

Membership of Congress may be obtained by sending 15 francs for full members (*membres titulaires*) and 10 francs for ordinary members (*membres participants*), with a visiting card, to the treasurer of the Congress, M. Pierre Masson, Boulevard St Germain 120, Paris.

Ordinary members (students of medicine or wives of full members, etc.) are entitled to the reduction in railway fares, but not to the publications of the Congress.

Recent Literature

CRITICAL SUMMARIES AND ABSTRACTS

MEDICINE

NEUROLOGY

By ASHLEY W. MACKINTOSH, M.A., M.D.,
Assistant Physician to the Aberdeen Royal Infirmary

A Note on the Influence of Maternal Inebrity on the Offspring (W. C. Sullivan, *The Journal of Mental Science*, July 1899, p. 489).—As the result of observations on the female population of Liverpool prison, the writer advances the following propositions, and gives notes of cases in illustration of them :—

1. Maternal inebrity is a condition peculiarly unfavourable to the vitality and to the normal development of the offspring. Its gravity in this respect is considerably greater than that of paternal alcoholism.

2. While its influence, particularly as measured by the test of infant mortality, appears to be exercised in considerable degree indirectly through deterioration of the milieu, a large part also depends on the primary action of the poison. The reality of this latter mode of influence is evidenced by the tendency to still-births and abortions, by the high rate of epilepsy in the surviving children, by the prevalent mode of death, by the effects of modifications of the intoxication.

3. This primary influence of alcohol is due in part to the permanent effects of the poison on the maternal organism, inducing a transmissible degenerate condition ; in part, to a direct toxic action on the embryo, owing to continued excesses during pregnancy and lactation.

4. The first of these modes of primary influence is, by its nature, permanent, with a tendency to increase. The second

mode, while tending also to a constant and constantly increasing operation, is susceptible of temporary augmentation or diminution.

5. Under these combined modes of influence the normal tendency of the family with alcoholic maternity is towards a type the inverse of the syphilitic family; that is to say, the first born children are normal, then come more or less defective children who live beyond infancy, then children dying in infancy, then still-births, and finally abortions.

6. Deviations from this type are probably due in many cases to oscillations in the intensity of the second mode of infection, *e.g.* death in infancy of the earliest born children of the family as a result of conception in drunkenness, and survival of late born children when the mother has been imprisoned during part of the pregnancy.

Congenital Facial Paralysis (H. M. Thomas, *Journal of Nervous and Mental Diseases*, 1898, p. 571, and F. W. Langdon, *Journal of Nervous and Mental Diseases*, 1899, p. 593).—Cases of facial paralysis dating from birth are rare, and their anatomical basis is unknown. From examination of the literature on the subject, Thomas finds fourteen cases of congenital facial paralysis combined with eye-muscle defect (especially in the region of the sixth nerve), and occasionally with defect in some other cranial nerves; in all these cases except one the defect was bilateral. In five cases there was uncomplicated, unilateral, congenital facial paralysis. In both these sets of cases the paralysis was as a rule incomplete, tending specially to spare the muscles which draw the mouth outward and downward. Thomas finds records of only two cases of uncomplicated bilateral congenital facial paralysis, but he gives notes and excellent photographs of two cases, brothers, who fall into this group. Various views have been taken of the pathology of this condition. Thus Möbius believes that congenital cases, as well as those acquired in infancy and childhood, are due to atrophy of the nuclei from which the nerves spring. The tendency is, however, to make a sharp distinction between the congenital and the acquired cases, and to regard the former as the result of defective development either in the seventh nucleus (Thomas), or anywhere in the motor tract from the cortex of the brain to the muscles (Kunn). There are no certain pathological facts to go upon, and there is moreover so great difficulty in excluding injury to the seventh nerve at birth as the cause, that Bernhardt concludes that "although the occurrence of an isolated, unilateral, congenital facial paralysis or, perhaps better, an incomplete development of the nerves and muscles in the

distribution of the facial nerve on one side, cannot be denied, still its occurrence has as yet not been definitely demonstrated" (Thomas). In the discussion on Thomas' paper, Spiller and Sachs suggested that the condition might be related to progressive muscular dystrophy of the Landouzy-Dejerine type.

Langdon records a case of unilateral facial paralysis, dating from birth, which he regards as congenital, and in which the distribution of the palsy was peculiar, affecting especially the occipito-frontalis, the orbicularis palpebrarum in its inner half, the elevators of the upper lip and the risorius. There was also some defect in the outward movement of each eye. Excellent diagrams and photographs are given. The possibility that this was a case of birth palsy, with incomplete recovery, naturally occurs to the reader.

Isolated affection of a particular nerve, occurring either as an independent entity or as a special symptom of a general nervous disease, is not uncommon in the arm, notably in the case of the ulnar nerve. It is less known in the leg. Within the last four years, however, considerable attention has been given to an affection in the region of the external cutaneous nerve, to which the name **Meralgia Paræsthetica** has been given (Roth). Musser and Sailer (*Journal of Nervous and Mental Diseases*, January 1900, p. 16) give notes of ten original cases, and find eighty-nine cases recorded in literature. Their definition is "a disturbance of sensation on the external surface of the thigh, characterised by various forms of paræsthesia, associated with dissociation and more or less diminution of sensation." In the great majority of cases the symptoms are limited to the region of distribution of the external cutaneous nerve on one or both sides. The subjective symptoms (tingling, numbness, aching, or even acute pain) are almost always worse when the patient is standing or walking, and may quite disappear during rest. These symptoms have, in some cases, been quite unbearable. Objectively there is usually unequal diminution of the various forms of sensation, pain and electro-cutaneous sensation being generally most profoundly affected; cutaneous hyperalgesia is often present, and in some cases there is tenderness on pressure just in front of and below the anterior superior spine of the ilium. The affection is a very obstinate one, often persisting for many years, and tending to recur after apparent cure. The application of faradism to the affected area, and, in a few very severe cases, resection of nerve, have met with success in treatment. While the affection seems to be clearly established as a distinct clinical entity, its pathology and ætiology are doubtful. Most writers agree that it is due to a lesion of the external cutaneous

nerve itself, and think that its special liability to affection arises from its superficial situation, which exposes it to injury, while the pressure of the fascia lata on the nerve may contribute to the result. In the great majority of cases no objective alteration (*e.g.* swelling) of the nerve has been detected. This and many other points in the symptomatology and course of the affection are against the generally adopted view of a neuritis of the nerve, although, in the only case which has come to autopsy, a swollen neuritic condition of the nerve was found. One author (Devic) suggests the possibility of a periostitis of the femur, while another (Venturi) locates the lesion in the cerebral thermo-perceptive centres. As regards ætiology, this affection is much more common in men than women (in Musser and Sailer's list seventy-five men, twenty-one women). This lends support to the view that trauma is an important ætiological factor. Infectious diseases, notably typhoid, and alcoholism seem also to have important relations. In a few cases the affection has been part of the symptom-complex of tabes and general paralysis. Musser and Sailer's paper contains an excellent account, as well as a full bibliography, of this interesting subject, which deserves the attention of the general clinician as well as the neurologist.

A paper by Drs Hughlings Jackson and J. S. Collier (*Brain*, 1899 Winter, p. 550) contains a number of most interesting observations on the **respiratory condition during the anæsthesia of chloroform and ether** in the case of both human beings and animals. In man, in the initial stage of chloroform-anæsthesia, there is increased range and frequency of the respiratory movement, which, however, remains normal in type or preponderatingly thoracic. Later, the elevation-expansion movement of the chest gradually becomes less and less, while the abdominal movement becomes more marked. After deep anæsthesia has lasted some time the respiration becomes entirely abdominal, except that the scalene muscles act for a little time longer, but later they cease to act. In this stage of purely diaphragmatic respiration the eversion of the lower ribs by the action of the diaphragm is marked during inspiration, while the upper two-thirds of the chest are passively sucked in during inspiration and recoil during expiration. With ether-anæsthesia the results are the same, except that the elevation-expansion movement of the thorax in inspiration is not completely lost, although much diminished. Very similar results are got in animals. It would seem, therefore, that in the coma of surgical anæsthesia, and perhaps also, as Dr Jackson suggests, in the coma of apoplexy, the manner of respiration differs according to the stage or degree of the anæsthesia. It tends to become more purely abdominal

the deeper the anæsthesia and the longer it lasts. In some peculiarly suggestive remarks Dr Jackson differentiates between respiratory and non-respiratory (cerebral, "voluntary") movement of the intercostal muscles. The latter is exhibited in the "voluntary" movement of drawing in the breath or expanding the chest on command. He refers to the different possible modes of action of the drug on the movements of the intercostal muscles. Of practical importance is his suggestion that more attention might be given to paralysis of the intercostal muscles in chronic disease of the nervous system. Such paralysis may be masked in various ways; by over-action of the scalene muscles, by the eversion and elevation of the lower ribs caused by the diaphragm, and by the reversed movement of the upper part of the chest, which so often occurs in cases of paralysis of the intercostal muscles, the chest being sucked in during inspiration and recoiling during expiration. Duchenne's statement that contraction of the diaphragm raises and everts the lower ribs *only* when it is supported by the abdominal viscera and parietes, was not confirmed either experimentally in an animal, or in the case of a girl, from whom a large abdominal tumour was removed.

It is well known that many cases of **facial spasm**—often a most obstinate affection—are of reflex origin, and are specially liable to occur from irritation of the fifth cranial nerve. Many arise from diseases of the nose and pharynx, but, in a large number of cases, some ocular defect, especially an error of refraction or muscular insufficiency, has been held to be the causal factor. It is this latter point which is emphasised by Stevens ("Facial Spasm and its relation to errors of refraction," *American Jour. of the Med. Sc.*, Jan. 1900, p. 33); he gives notes of cases, in some of which distinct benefit was derived from treatment of the ocular defect. In such cases he emphasises the following points:—"(1) the refraction and muscle-balance should be carefully examined; (2) the refraction should be determined under complete mydriasis and the full correction ordered; (3) it is not sufficient to get rid of the cause, for the nerve-centres, having acquired a vicious habit, do not recover their normal condition until systematic treatment has been pursued" (*e.g.* arsenic in increasing doses).

The **relation of migraine to epilepsy** is a question of great interest. Both diseases not infrequently occur typically in the same family, and even in the same individual. As we are ignorant of the pathology of either, the distinction must be based solely on clinical grounds. The greatest difficulty occurs on the one hand in cases of migraine in which the headache

and characteristic visual disturbances are slight or absent, and the principal symptoms are sensory disturbances (paræsthesiæ) and aphasic troubles; and, on the other hand, in cases of minor epilepsy, with no convulsions and only slight disturbance of consciousness, and in which the sensory discharge is the most conspicuous feature of the attack; in both cases a transitory paresis may accompany the paræsthesiæ. In some cases, indeed, it may be impossible to say whether epilepsy or migraine is present. Two excellent cases illustrating this difficulty of diagnosis are reported by Spiller (*The American Journal of the Medical Sciences*, Jan. 1900, p. 24). In both cases there are attacks of unilateral paræsthesiæ and paresis with disturbance of speech. The first case is that of a man, age fifty-one, who between the ages of forty-four and forty-nine suffered from attacks—lasting a minute or two—of numbness, at first confined to the right side of the tongue and accompanied by inability to speak; later, the numbness spread to the right hand and arm (with transient paresis of the part), and, later still, affected the right leg also. There was no loss of consciousness, no headache, no visual disturbance. After these attacks had lasted five years he began to have major attacks with aura, a sort of cry, loss of consciousness, but no convulsions (except, perhaps, in the last). Spiller is inclined to look on this as an example of sensory epilepsy. The second case is that of a woman, age twenty-one, who, from the age of four and a half years, suffered from attacks of intense unilateral headache—now on one side, now on the other—with transitory paræsthesia and paresis on the opposite side of the body and paraphasia, the attacks lasting about half an hour; there were no visual disturbances. Spiller inclined to the view that this was a case of migraine, but a "possible relation" to epilepsy was shown, when the patient was twenty-one years of age, by an attack of "doubtful interpretation"—severe headache, apparent unconsciousness, and reputed convulsive phenomena. These and similar cases emphasise the important fact that "a person who has ophthalmic migraine, with aphasia and unilateral paræsthesia, may some day develop Jacksonian convulsive attacks." Spiller sums up his views on the subject in five propositions, of which these two are specially worthy of attention:—"Migraine, especially the ophthalmic form, is related to epilepsy, and the attacks of migraine may precede for many years the convulsive attacks of epilepsy, although in most cases of migraine no convulsions are ever detected." "In some cases epilepsy appears in the form of one or more of the disturbances seen occasionally with migraine, and later, even after many years, convulsions develop. The disease may be epilepsy from the beginning. It matters little,

with our uncertain knowledge of the pathology of the two diseases, whether we regard these as abortive cases of migraine that later become associated with epilepsy, or as abortive forms of epilepsy (sensory epilepsy) in which the convulsions later become apparent, provided we recognise a relation between some forms of migraine and epilepsy."

Cerebral Diplegia.—Few subjects have received more attention within recent years than that of Cerebral Diplegia, or Little's Disease. Beyond a more detailed classification of the various types, little has been added to the *clinical* description given by Little. Regarding the ætiology and pathology, however, widely varying opinions have been, and are still, advanced, and it is as a contribution to the solution of these problems that we welcome Collier's excellent paper, which is enhanced in value by the fact that it contains the records of the pathological examination of two cases performed by such a skilled and careful observer as Risien Russell.

The first part of the paper contains the records of seven cases observed in the National Hospital. The first four cases are examples of generalised rigidity, beginning at periods after birth varying from 2 months to 2¾ years; the fifth is a case of paraplegic rigidity, in which symptoms began at the age of 4 years; the sixth is a case of generalised rigidity, beginning at the age of 16, after epilepsy of many years' duration; the seventh is taken to be an example of the rare occurrence of generalised rigidity in an adult, the symptoms beginning at the age of 25.

In two cases, the histological examination of the central nervous system showed great paucity and marked atrophy of the pyramidal cells of the cortex, with descending degeneration in the pyramidal fibres, without a trace of inflammatory changes, sclerosis, or softening in the cortex or any other part of the central nervous system. The pathology of these two cases is therefore held to be a slow primary degeneration of the cortical neurons. Collier believes that the same is true of cases six and seven.

The classification adopted of the forms of cerebral diplegia is a clinical one, following that of Rosenthal: —

(1) Generalised rigidity; (2) Paraplegic rigidity; (3) Bilateral spastic hemiplegia; (4) Bilateral athetosis; (5) choreiform diplegia (congenital chorea); (6) Atypical forms (slight generalised rigidity with profound mental changes).

Collier deals in considerable detail with the clinical aspects of these different forms; we shall confine our attention to his conclusions as to the ætiology and pathology of the condition.

ÆTIOLOGY.—He finds that one-third of the recorded cases of cerebral diplegia are without known clinical antecedents.

Maternal morbid states during pregnancy (*e.g.* general ill-health, pathological psychic states, acute diseases and specific fevers, syphilis, trauma to the fœtal head *in utero* causing meningeal hæmorrhage) are "probably by far the most important clinical antecedents, and such states have probably existed in many of the cases to which no definite cause can be assigned."

Abnormalities of birth, viz., premature birth, birth injuries, precipitate labour, and asphyxia neonatorum. "Premature birth has probably no causal relation with diplegia." He emphatically opposes the view advanced by V. Gehuchten, and accepted by many French writers, viz., that premature birth arrests the development of the pyramidal tracts, and is thus the cause of the diplegia; he believes that both the premature birth and the diplegia are concomitant effects of some morbid process affecting the mother during the pregnancy. "The causal relation of birth injuries and asphyxia neonatorum to this condition has been much over-estimated, and the absolute proof is as yet wanting, notwithstanding the frequency of the clinical association." He gives reasons, based on both clinical and pathological grounds, for his objection to the general acceptance of birth injuries and the consequent meningeal hæmorrhage as causal factors in the production of diplegia. He quotes Freud's view, that difficult birth and asphyxia neonatorum are only symptoms of deeper lying influences which have dominated the development of the fœtus or the organism of the mother.

Post-natal ætiological factors.—"Some cases (of diplegia) certainly follow cerebral vascular lesions" *e.g.* thrombosis, multiple embolisms, multiple hæmorrhages. "Acute diseases (*e.g.* various specific fevers) sometimes precede the onset of symptoms, but whether in all cases as the initial factor in disease, or in some cases only as an immediate exciting cause, has not been determined."

Speaking generally, Collier argues for a congenital origin, due to some toxic state, in the great majority of cases of diplegia, even when (as in his case five) no symptoms are apparent for several years after birth, and when they appear suddenly as the result of some acute illness.

Pathology and Morbid Anatomy.—This has been the subject of much controversy, due mainly to the fact, as Collier points out, that the pathological examination of most cases recorded has not been made till long after the onset of the disease and it has not been possible to deduce with certainty the initial lesion from the final lesions found. These final lesions have

been mainly two: (1) diffuse and lobar atrophic sclerosis of the cerebral convolutions, and (2) porencephaly. In not a few cases, however, examined pathologically at a comparatively early stage of the disease (*e.g.* Collier's first two cases), no coarse lesion has been found—no evident atrophy of the convolutions, and no sclerosis; there has been nothing but degeneration and disappearance of the cortical neurons. These cases lead him to the opinion that "too much stress has been laid perhaps on the coarse anatomical lesions, which may be only secondary results of primary degeneration in the nerve elements themselves, and these last factors alone may determine the symptomatology." This view of the primary pathology of cerebral diplegia is, he believes, most in accordance with the clinical aspect of many cases of the disease, especially with the toxic element which, according to his view, preponderates in the ætiology—toxic processes having a preference for the nervous elements themselves, rather than the interstitial tissue.

With regard to other views which have been advanced regarding the nature of the initial lesion, he allows that some cases of diffuse and lobar sclerosis have been due to vascular lesions, viz., embolism (especially in infantile hemiplegic states), thrombosis, and intra-cerebral hæmorrhage; he thinks that meningeal hæmorrhage has been proved to be common in young children, but that it has not been "conclusively proved" that this can produce cortical atrophy and the symptoms of diplegia. Polioencephalitis, which Strümpell has so strongly advocated as the cause of both sclerosis and porencephaly, "seems to have no connection with diplegia."

Porencephaly may be produced by (1) arrest of development with tissue-absorption owing to fœtal vascular disease; (2) direct injury to the brain; (3) vascular lesions after birth—embolism, thrombosis, and intra-cerebral hæmorrhage; (4) it may also result from the process of tissue-absorption and shrinking of the brain substance occurring in cerebral sclerosis, and may result as a secondary process in degeneration of the cortical neurons.

Many other points of great interest are most ably discussed in the paper, and a very full bibliography is appended.

The facts adduced by Collier seem to prove it certain that primary degeneration of the cortical neurons is an important factor in the pathology of certain cases of cerebral atrophic sclerosis. It is difficult, however, to believe that such an initial lesion can be responsible for any marked degree of porencephaly, and even Collier only holds it may "possibly" cause "some porencephalic defects, and the associated atrophy of the convolutions." Now, the fact that porencephaly is, as a rule,

associated with a condition of the convolutions identical with that found in pure atrophic sclerosis (with no porencephaly), seems to prove that the same cause is capable of producing both the atrophic sclerosis and the porencephaly. If we attempt to explain these combined cases on the basis of a toxic condition causing primary degeneration of the cortical neurons, it seems more probable that the porencephaly is the expression of the concomitant effect of the toxic process on the interstitial tissues (probably of vascular nature), than that it is simply secondary to the primary neuron degeneration.—J. S. COLLIER, "Brain," Autumn 1899; pp. 373-441.

GASTRO-INTESTINAL DISEASES

By G. LOVELL GULLAND, M.A., B.Sc., M.D., F.R.C.P.Ed.,
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WE have all been accustomed to consider that bad teeth are a frequent cause of stomach disturbance, both because of the imperfect chewing of food which results from them, and because they harbour organisms which, when swallowed, set up gastritis; but it will surprise many people to find that Dr William Hunter (*Lancet*, January and February 1900) believes that **bad teeth are the cause of pernicious anæmia**. He considers that this disease is a special form of chronic blood-poisoning due to a special infection of the digestive tract, the chief source of which is through the mouth, in connection with long-continued and neglected cario-necrotic conditions of teeth (though sometimes possibly from stomatitis from other causes). The usual effect of this infection, which is chiefly streptococcal and probably derives its special characters from being of a "mixed" character, is a chronic infective catarrh of the mouth and stomach which may in time lead to deeper seated changes, *e.g.* ulcers of the mouth and tongue, chronic glossitis and atrophic changes in the tongue, and chronic gastritis with atrophy of gastric glands; and the excessive destruction of blood in the portal area is the result of poisons absorbed into the blood. This theory is certainly new and startling, but, as far as my not inconsiderable experience of the disease goes, is probably without foundation.

Bottermund (*Therapeut. Monatsh.*, March 1900) has written an interesting paper on the **pathological relations between the upper air-passages and the digestive organs**. He points out that diseases of the nose, such as adenoids, polypi, and different forms of rhinitis, which cause mouth-breathing, make it impos-

sible for the sufferer to chew his food as thoroughly as he should do, since he must stop his chewing in order to breathe. The food is imperfectly mingled with saliva and the digestion of starches is incomplete, and moreover the stomach is put to harder work in digesting the badly-chewed food than it can manage. In the long run this leads to a chronic gastric catarrh with increased secretion and hyperacidity. In other conditions, such as ozæna, where there is a purulent secretion from the nose, this is swallowed, especially during the night, and gives rise to gastric catarrh, either chemically by the decomposition-products of the pus, or from the pathogenic activity of the organisms present in it. It is, of course, well known that patients with ozæna often complain of gastric symptoms and look ill and anæmic to an extent out of all apparent proportion to the local mischief, and that one of the first results of local treatment is an improvement in digestion and in general appearance. A still further cause of gastric disturbance in nasal diseases is to be found in the loss of smell, and, secondarily, of taste, which so often accompanies them. Food with a pleasant taste is not distinguished from that with an unpleasant taste, and an unpleasant taste is, of course, often associated with undesirable food. Moreover, suppurative nasal disease is frequently associated with a continuous unpleasant subjective odour, which is wont to lessen the patient's appetite and so to interfere with nutrition. Bottermund points out also that gastric disorders may produce chronic hyperæmia of the nasal mucous membrane with all its unpleasant consequences, and such conditions as granular pharyngitis. It is not necessary here, however, to enter into a discussion of the way in which these are brought about.

Pfaundler has recently produced two very interesting studies in digestion, both of which deserve mention. The first (*Wiener klin. Wochenschr.* 1899, No. 41) deals with **digestion in infants**, and especially with the reason why breast-babies are, on the whole, healthier than bottle-babies. There is, of course, no doubt that the difference in chemical composition between human milk and cow's milk is mainly responsible for this. But Pfaundler believes that the difference in the way in which the act of sucking is performed in the two cases is also of importance. The child which sucks at the breast does a certain amount of work in the movements of the lips, tongue, and jaws which are necessary, and this ensures that the child gets tired at about the right time and stops sucking. In this way an injurious overfilling of the stomach, which is common in bottle-babies, does not occur in breast-babies. The ordinary bottles

don't make it necessary for the child to suck nearly so vigorously as if it were at the breast. There is another point also. The author gave several infants the same food. One set received it from the bottle in the ordinary way, so that they had hardly any work to do in sucking; the other set were given teats which made it necessary for them to suck energetically in order to get their food. When the contents of their stomachs were examined, it was found that the secretory activity of the stomach was greater in the latter set, probably because the act of sucking excites the secretion of gastric juice, either reflexly or through an increased secretion of saliva. The obvious moral is that bottles should be so arranged that infants don't get their food too easily.

Pfaundler's second paper (*Deutsch. Arch. f. klin. Med.*, vol. lxv., Heft 3 and 4) is on a new method for the **clinical investigation of the functions of the stomach**, and is characterised by a truly German minuteness and mathematical exactness. The idea with which he started was whether it would not be possible to find a measure of the secretory and motor activity of the normal stomach which should be universally applicable and should serve for the comparison of function between normal and diseased organs. He finds that when a test meal of measured amount is ingested and the hydrochloric acid estimated in samples of the mixed stomach contents taken at definite intervals, it is possible to determine (*a*) the total amount of gastric juice secreted, and the amount poured out at different stages of digestion; (*b*) the length of time during which the juice secreted contains hydrochloric acid; (*c*) the amount of acid contained in this secretion; (*d*) the amount of the stomach contents passed on into the intestine at different periods of digestion. In stomachs whose chemistry was normal, he ascertained the following data. (The test-dinner consisted of soup, meat, potatoes, bread and water; the test-breakfast was the usual one.)

	Test-dinner.	Test-breakfast
Total amount of gastric juice secreted	595·5 c.cm.	105·5 c.cm.
Duration of secretion about	4 hours.	1½ hours.
Percentage of hydrochloric acid	0·3514 per cent.	
Contents passed from the stomach into the intestine	each hour, nearly steadily, an average of 281·6 c.cm.	in half hours successively, 99·1 — 55·3 —11·4 c.cm.

The healthy stomach secretes the greatest amount of gastric juice in the first hour or half-hour after a meal, thereafter the amount steadily diminishes. In the digestion of a test-dinner by the normal stomach, lactic acid (and perhaps other organic

acids) is formed by fermentation, and its amount in the mixed contents may be as much as 0·1 per cent. The acid stomach contents are normally gradually neutralised towards the end of digestion by an alkaline secretion poured out into the pyloric end of the stomach, and probably derived from the pyloric glands. In hyperacidity (at least in one type of this condition) more gastric juice is secreted in a given time, and the secretion lasts longer than normal. Probably, however, an imperfect or delayed neutralisation of the stomach contents towards the end of digestion also plays a part in this disease.

The statistics which have been published of the results of **operation on perforated ulcers of the stomach and duodenum** show that from one-fourth to one-third of the cases recover; and that the percentage mortality of the cases operated on within twelve hours from the beginning of the symptoms is thirty-nine, while it is seventy-six for those operated on between twelve and twenty-four hours.

Lennander (*Mitth. a. d. Grenzgeb. d. Med. u. Chir.*, vol. iv., Heft 1) reports fifteen new cases with very much the same result as the above, and discusses the whole question. The first condition necessary to the saving of a still larger number of patients is that practitioners should give up thinking that in these cases it is their first duty to relieve pain. They should rather, with the help of the pain, make up their minds whether operation is to be immediate, or whether it is to be delayed, or not performed at all. Even when operation is to be done at once it is best not to give morphia, or at most a small dose, in view of the possibility of paresis of the intestine after the operation. Shock is best treated by immediate interference; and another reason for early operation is that any considerable amount of meteorism makes a proper cleansing of the peritoneum impossible. The prognosis depends most on the time which elapses before operation takes place, and then on the amount and nature of the gastric contents which have escaped into the peritoneum, the violence and extent of the inflammation which has been set up, and further, how successful the cleansing and drainage of the peritoneal cavity has been. In cases where, for any reason, operation has not been performed and the patient survives, *nothing* is to be given by the mouth for a week, even though the patient improves so much that the accuracy of the diagnosis is called in question.

Galli-Valerio (*Therap. Monats.*, March 1900) draws attention to the possibility of using **salol as an anthelmintic**. He gave 15 grains to a patient who had a tapeworm, and a few hours

afterwards the entire worm—a *bothriocephalus latus*—was found in the stools. The explanation probably was that the breaking up of the salol in the intestine into carbolic acid and salicylic acid made the worm so uncomfortable that it sought other quarters.

GYNECOLOGY AND OBSTETRICS

By R. C. BUIST, M.D., M.R.C.P.,Ed.,
Gynecologist to the Royal Infirmary, Dundee

Mechanical methods of treatment in gynecology.—(*a*) The success of massage and gymnastics in the removal of chronic inflammatory swellings and stiffness in general surgery has often suggested analogous treatment in gynecology. The difficulties in the way are, however, so considerable that manual methods have little favour in this country, but on the Continent and in America their advocates are as eminent, if less out-spoken than their opponents. In the discussion some modifications have emerged which promise, and if the reports of results are confirmed, prove to be useful. Freund having seen an incarcerated omental hernia reduced by a bag of shot, applied the method to the pelvic organs by placing a stall of No. 3 shot in the vagina and a larger bag on the abdomen. The rubber sheath which goes into the vagina is drawn over a medium Fergusson speculum which is then put into the vagina and withdrawn as the shot is poured in and packed into the cavity. A pound or two of shot can be introduced. After the method had been in use for fully a year, Freund's cases were reported by Funke (*Beiträge z. Geburtsh. u. Gyn.* 1898, Bd. i., p. 263). Exudations seem to be compressed like a wet sponge. In three hours 600 gm. in a woman of sixty-two compressed a mass that bulged forward the vaginal wall, so that the normal concave form returned. It was, of course, not then finally disposed of. A retroflexed gravid uterus at the third month was replaced in one sitting, and without anæsthesia, and old and resistant retroflexions were replaced by directing the bag of shot into the anterior fornix. In cases of old inflammatory bands, good results are got often in a few sittings; while pain in coitus disappears where it is due to tenderness of the muscles or to inflammatory contractions in the broad ligament. Pus is not, of course, got rid of in this way, but tumours may be pushed up in the pelvis and made more accessible, and the method may help in the diagnosis of the fixation of a tumour, or serve to distinguish an inflammatory

swelling from a new growth. The method is contra-indicated in acute cases, and when it causes fever or severe pain. In these cases it is probable that pus may be present without having been recognised. A tabular report of sixty-six cases is given. Pincus has employed the shot bag externally, but distends the vagina with an air bag, as Auvard had also done, and he uses the raised pelvis position.

(*b*) Schauta has made an excellent modification of this loading method by using a rubber bag with mercury for introduction into the vagina in place of the shot. The bag, a Barnes bag, or similar form, is introduced into the vagina, and the mercury is easily poured into a funnel at the end of the tube. The weight of metal used can be much greater, both on account of the higher density of mercury and the closer packing of the fluid as compared with shot. Three pounds can be used in suitable cases. The bladder being emptied the bag is introduced, the patient lying in the high pelvis position so that the pressure is directed towards the pelvic organs rather than against the rectum and sacrum; by putting the patient partly on her side and placing the external load to correspond, a unilateral affection can be treated. The duration of the pressure varies with the tolerance and habituation of the patient, from an hour to the entire day. Urine can be passed while the bag is still in the vagina. Halban (*Monats. f. Geburtsh. u. Gyn.* 1899, Bd. x., p. 128) reports the cases in which Schauta has used the method. (1) Chronic tumour of the appendages. In these, owing to the occasional presence of still virulent organisms, there is a risk of causing exacerbation. It is, therefore, necessary to make the difficult diagnosis of their absence, and in doubtful cases to begin the treatment with great moderation, and to add to the weight of mercury used and the elevation of the pelvis as the treatment is well borne. In acute cases, Schauta has never used the loading at all. In cases where the treatment is tolerated, the prognosis is not uniform; swelling may diminish without relief of pain, and pain may be relieved without perceptible change in the swelling. (2) Perimetritis.—The result varies with the seat and accessibility of the adhesions and contractions. When these are low in the pelvis the effects of treatment are good, and in cases of dyspareunia especially, due to contraction in the sacro-uterine ligaments, sometimes brilliant results follow two or three seances. (3) Cicatricial bands due to cervical tears also give good results. (4) In Parametritis the most brilliant results are got in cases with the most extensive objective affection. Where the pouch of Douglas was full of hard immovable masses, two or three sittings quite changed the condition to palpation. This often

permitted the discovery of adnexal tumours which had previously been suspected only. (5) In retroversion of the uterus, very good results are to be obtained, and Halban thinks this likely to be the most permanent achievement of the method. As in Funke's paper a good case of reposition of the gravid uterus is reported. A tabular report of thirty-five cases is given.

(*c*) At the end of his paper Halban attempts to define the relations of the loading treatment to massage, and says that the loading while being more comfortable for the patient and convenient for the physician, is also quicker and more effective for conditions low in the pelvis, but that massage has to come in for affections at the level of the fundus uteri or higher. In the Schauta Festschrift (*Monats. f. Geburtsh. u. Gyn.* 1900, Bd. xi., p. 260) Piering discusses in the favourable sense of Schauta's pupils the Thure Brandt method, indicating the present literary position of the question. The contra-indications to the method are—(1) all acute and feverish conditions; (2) gonorrheic and septic pus collections; (3) general conditions such as tuberculosis; (4) neoplasms; (5) nervous excitability. The necessary conditions are an absolutely exact diagnosis, and exact technical skill in the performance of the massage and exercises. The treatment should never cause sexual excitement or pain. From this it is evident that the method could never be a safe one in the hands of one not specially trained. The conditions for which the method is suited are—(1) chronic inflammation of the cellular tissues; (2) flexions and versions of the uterus; (3) chronic metritis; (4) dislocation and fixation of the ovaries; (5) old hæmatocele; (6) relaxation of the pelvic muscles and its consequences; (7) sterility dependent on the residua of former affections. In my own experience the prescription of systematic exercises for the pelvic and abdominal muscles has given most useful results in the treatment of some cases of prolapse and in movable kidney. It is not improbable that if these were employed before operation, the percentage of good results would be much higher than it is in some of the records.

Anatomical Evidence of Defloration (*Monats. f. Geburtsh. u. Gyn.* 1900, Bd. xi. p., 69).—Haberda, the Professor of Medical Jurisprudence in Vienna, gives the results of his experience during five years as medical referee to the Vienna Courts on this question. In one hundred and fifty cases he has only once seen a recent injury of the hymen, and he points out that later the evidence may be absent even when there has been complete penetration, owing to no injury having occurred when the hymen is sufficiently distensible, or to the evidence having disappeared by the time the examination is made. In forty cases

of rape, where there had confessedly been penetration, no anatomical evidence was present in fifty per cent. When the hymen is torn, cicatricial adhesion of the torn edges is unusual. Haberda has only seen it once in his cases. It is not always possible to distinguish the indentation of the edge due to injury from the congenital indentation; the scar at the edge may be too fine for vision, and asymmetry occurs in both. Almost all lobation of the hymen is related to the columnæ rugarum of the vagina. A division that passes through the base of the hymen on to the vaginal wall, be it ever so slightly, is unmistakable evidence for injury. Illustrative cases are given, and specially some interesting experiments on the distensibility of the hymen.

Deciduoma malignum.—1. Emil Müller (*Hospitalstid.*, 1899, p. 539). Patient, aged twenty-eight, after two normal pregnancies had a vesicular mole delivered at four months in March. In June she had had frequent bleeding which had resisted rest, ergot and douching, and had lost strength and flesh. On 1st July 1898 the large anteflexed uterus was dilated and scraped and some masses removed, including several vesicles. Microscopic examination of the material showed marked malignant degeneration and proliferation of the decidual cells, and to a less extent of the syncytium. Pieces showed malignant infiltration of the muscle. 15th July, vaginal hysterectomy, with clamping of the ligaments. Patient did well, and went home on 7th August. On 12th December 1898 she wrote that she was entirely well and had recovered her strength. The preparation was handed over to a colleague for detailed examination, and the result is to be reported later.

2. Jörgensen, from Howitz's Wards (*Idem.*, p. 791). Patient, aged forty-nine, had the last of a series of abortions and five normal pregnancies, eight years before. The periods had continued, and become freer and a little earlier, but had been regular till four weeks before, since which bleeding had been continuous and latterly, in spite of rest, very free, and she had had giddiness, headache and vomiting. On admission the uterus was fist-sized. A wineglassful of tissue was removed with the curette and the uterus stuffed with gauze. Examination of the material showed chorionic villi, and the characteristic parts of placenta and decidua with various types of cells showing a tendency to irregular proliferation and sarcomatous change. A second scraping a month later showed a distinct advance in the malignant characters, and the uterus was extirpated by Doyen's method. The convalescence was feverish, but patient was able to go home after five weeks. She was well a year after operation. The tumour mass was principally a nodule

in the left half of the uterus. These are the first cases from a Danish source.

The Influence of Sugar in Pregnancy and Labour.—In the original paper on the influence of sugar on muscular work, Mosso and Paoletti (*Arch. Ital. de Biol.*, 1894, T. xxi., p. 293) using the ergograph and working with muscles pretty well exhausted, reached the following conclusions: (1) dilute solutions give best results, the maximum being got with solutions of sugar in 5-10 times its weight of water; (2) small and medium doses (5-60 gm.) give best effects, diminishing the dose to 5 gm. shortens the duration of the work but does not affect its intensity; (3) the improvement is evident in ten minutes after the administration of the sugar, the maximum effect appearing immediately for small doses and after 30-40 minutes for medium doses; (4) with doses repeated at short intervals one may replace the energy which the muscle is using in its work. At the conclusion, they say, "we hope that this may be employed with advantage in the field of therapeutics, for example, for reviving the uterus in labour when, fatigued by vain efforts, the expulsion of the products of conception is yet urgent." Bossi (*Idem.*, p. 300) took advantage of the suggestion in eleven cases of inertia uteri, absolute or almost absolute, giving 30 gm. in 250 gm. of water, and repeating the dose in one or two hours when the effect was insufficient. He found in four-fifths of the cases a positive ecbolic effect, produced on the average in about half an hour and sufficing in several cases for the expulsion of the fœtus. A second dose accentuated the action and the contractions which followed were never tetanic but regular, and there was no retention of the placenta. The most recent paper by A. Payer (*Monatss. f. Geburtsh. u. Gyn.*, 1899, Bd. x., pp. 559, 784) contains a most imposing array of observations on the production of alimentary glycosuria in pregnancy and on the influence of sugar on labour. It is unfortunate that the author has not been able to resist the temptation to generalise from insufficient data, for though he has pointed out the scantiness of his material, he draws conclusions from averages of even two cases, of which at times one has given a negative result. While, therefore, the first part of the paper indicates that there is a more ready production of alimentary glycosuria in pregnant women, his deductions as to the effect of multiparity and other conditions must await demonstration of a more patient character. In particular it will be needful to determine the lower limit of dosage for each patient before making comparisons such as he does between his cases in pairs. The introduction of rum as a medium for the sugar must also not be assumed to be an indifferent factor in

the problem. The observations during labour deal with thirty-three cases. Seven of inertia towards the end of labour, receiving doses from 30-100 gm., showed the effects in from 15-60 minutes, the last being in a case of administration by enema. The evidence of the other cases, including six of the administration of large doses before labour, is not very cogent as to the definite efficiency of sugar. The author says that the sugar had a considerable effect in alleviating the painfulness of the uterine contractions. He seems to have given doses larger than those recommended by Mosso, and it is probable that in the labour cases also it was given in rum and tea, both of which have credited to them an effect on the vigour of uterine contractions. For practical purposes this might be excused, but for a scientific investigation, both should be excluded. Clinically, it would seem best to give the sugar in doses of 15-30 gm. (half to one ounce) repeated if needed every half hour. The cases which promise most result are those of secondary inertia.

Experimental Researches on the Effects of different Anæsthetics on the Kidneys (R. C. Kemp, *New York Med Journ.*, 1899, vol. lxx., pp. 732, 765, 804).—In investigating the effects of serum and antitoxine injections on the renal circulation, it was found that ether anæsthesia had a specific effect which had to be eliminated. Kemp now in this important paper reports some further work devoted to the influence of the anæsthetics. The investigation was made with the oncometer, and a series of typical tracings of the kidney volume and blood pressure curves is given.

The more important results are—(1) Ether.—The first effect is an early diminution in the number of drops of urine from a catheter in the ureter of the undisturbed kidney: the urine decreases progressively as the oncometric curve drops, till under free and continuous etherisation the secretion is suppressed. Albumen appears early even under moderate anæsthesia, and increases greatly under deep narcosis. (2) Chloroform.—The renal secretion remains copious till the general circulation is much depressed. Albumen appears only after prolonged narcosis and in but small amount. (3) ACE mixture.—By the open method the blood and kidney tracings are those of chloroform, by the semi-closed method the kidney tracing becomes that of ether. The effect is modified by the dilution of the ether, and complete suppression of urine does not occur, and albumen appears later than with ether and is more than with chloroform. (4) Schleich's mixture.—The tracings and effects are between those of ACE mixture and pure ether. (5) Nitrous Oxide.—The circulation is affected as in asphyxia and the kidney is

secondarily affected, the results being rapid diminution in the renal secretion and a transient moderate albuminuria. (6) Anesthol.—This ethyl chloride mixture gives respiratory embarrassment all through, seriously depresses the circulation, and gives early and complete suppression of urine. A few clinical tests agree with these conclusions, but further investigation on this side of the question is needed.

Dermoid Tumours of the Abdomen (R. Hertz, *Hospitalstid.*, 1899, p. 1215).—(1) *Multiple Dermoids.*—Patient, aged thirty-nine, who had children in 1885 and 1888, in 1890 was operated on for tumour. This was a thin sac adherent to the parietal peritoneum and bowel, and tore, giving issue to an odourless yellow fluid. On the intestine at many parts were firm yellow nodules, the largest about the size of a large nut, and in the right parametrium was a soft sac the size of an apple. As much of the large sac as possible was excised, and the abdomen washed out with boric lotion and closed. The fluid and excised nodules showed only detritus and fat and no trace of malignancy. She did well, and on dismissal there was felt a walnut-sized tumour partly in, partly above the right parametrium. In 1893 she had a child which she nursed, and at this time there were no tumours. In 1899 she was admitted for a large ventral hernia in the scar. There was a small tumour in the scar, and behind and to the left of the uterus a partly cystic, partly solid tumour the size of an ostrich egg. For this operation was performed. The large tumour was a dermoid with a large bone like an upper jaw with many teeth. The small was loculated, and contained detritus, some hairs, and lime crystals. The author could find only two cases of multiple dermoid on record, those of Fraenkel and Kolaczek. (2) *Mesenteric Dermoid.*—Patient, aged twenty-six, had one child two years before, and was admitted with acute peritonitic symptoms, nausea, hiccough, bilious vomiting, and temperature 101° F. There was a tumour resembling a kidney in the left half of the epigastrium. It was removed and proved to be a dermoid with yellow chalky content seated on a loop of small intestine and adherent to another loop. Patient did very well.

OPHTHALMOLOGY

By J. B. PATERSON, M.B., F.R.C.S.E.,
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Optic Neuritis and Acute Myelitis (*v. Graefe's Archiv.*, Bd. 48, Abt. 3).—Dr Albin Dalén of Stockholm, working in Professor Fuchs' laboratory in Vienna, publishes an account

of a case where acute myelitis was preceded by optic neuritis. The neuritis was not of the nature of "choke disc," but consisted merely of a slight striation and blurring of the disc margins with swelling of the veins. From the first there was great interference with vision, and by the time the myelitis developed, or about a month from the first appearance of the eye symptoms, the patient had become completely blind. The myelitis proved fatal in about three weeks, and at the *post-mortem* a typical acute myelitis, especially of the lumbar and dorsal regions, was found. The medulla and brain were healthy.

Dr Dalén examined the optic tracts, chiasma, and optic nerves. He employed Machi's and Weigert's methods, and also Mallory's method to demonstrate increase of the neuroglia. In the intracranial portions of both optic nerves, and in the chiasma and adjacent portions of the optic tracts, the medullated fibres were found to have completely disappeared, and the interstitial elements to have greatly increased in amount. In the orbital portion of the right optic nerve most of the medullated fibres were intact, except in a small area near the eye. The case was investigated bacteriologically, with negative results. The author draws attention to the sharp line of demarcation between the diseased and healthy portions of the right optic nerve, as perhaps explaining the occurrence of definitely circumscribed defects in the visual field in retrobulbar neuritis.

Ophthalmia Neonatorum due to Bacterium Coli Commune (*Zehender's Clinische Monatsblätter*, September 1899).—Dr Amilcare Bietti of Parma publishes from the University Clinic in Rostock, a case of typical ophthalmia neonatorum of moderate severity, in which the bacteriological examination showed a pure culture of the above organism, the bacterium having apparently been most carefully identified. Very few cases of conjunctivitis due to this organism alone are on record.

A Contribution to our Knowledge of Choroidal Sarcoma (Dr E. Pawel, Halle, *v. Graefe's Archiv.*, Bd. 49, Abt. 1).—The author cites and tabulates a hundred cases observed at the Halle Clinic during a period of twenty-five years. Statistically, Dr Pawel's results correspond very closely to those given by Fuchs and other observers, as, for instance, in the age of greatest predisposition, fifty to sixty years. In discussing the ætiology, Dr Pawel shows how very few of the cases ascribed to injury stand a searching criticism. As regards the rare cases where choroidal sarcoma is accompanied by in-

flammation of the whole uveal tract ending in atrophy of the globe, the author gives prominence to Leber's view that the inflammation is due to infection with organism perhaps endogenously, the exudative and necrotic processes induced by the sarcoma forming an excellent nidus. His statements regarding prognosis bear out those of other observers, showing that the danger of local recurrence is extremely slight where the growth is still intra-ocular before enucleation, while at the same time the danger of metastatic deposits is not sensibly lessened by early operation. In fifteen cases cited by Pawel in which the eye was enucleated while the growth was yet in the first stage (*i.e.* before increased tension had appeared), no less than eight cases subsequently developed metastases in other parts of the body.

Of the hundred cases, twenty are described as leucosarcomata containing very little or no pigment.

Notes on Foods, Drugs, &c.

Messrs Duncan, Flockhart & Co. have forwarded us samples of their **Glycerophosphate** preparations.

The Syrup is a bright, clear preparation, easy to take, being almost destitute of the sickly flavour so common in medicinal syrups. They have modified, they think for the better, Dr Robin's original formula by the substitution of caffeine for kola, while the place of the diastase in his preparation is taken by a fluid malt extract in theirs. This combination with malt extract seems to increase the efficiency of the Glycerophosphates, and as a tonic after influenza we have used it with good effect.

Messrs Beiersdorf & Co. have made for Dr Blaschko, of Berlin, what is translated into English as a **Mercolint "Bib"** (Ger. Schurze). It consists of a cotton wool fabric impregnated with mercury, and when worn by the patient the mercury is volatilised, inhaled, and exerts its full therapeutic effects. Inunction is not, in this country, a popular method of administering mercury. It is, though admittedly efficacious, dirty, troublesome, and impossible to conceal from at least some members of the household. This Bib has almost all the advantages and none of the disadvantages of that method. It is not usual for an advertisement to claim less than the merit which the preparation deserves, but this seems to us to do so, for it has proved at least as efficacious in primary cases as any of the ordinary methods, while the results in some recurrences have been strikingly successful. All the patient has to do is to wear the bib, and in a few days the effects of the absorption of the mercury are observed; in one case in which it was tried salivation was produced. We have every confidence in recommending it as one of the most important recent improvements in the Therapeutics of Syphilis.

NOTICE.

All communications to the Editorial Department, including Exchanges and Books for Review, should be addressed to THE EDITOR, Dr W. RUSSELL, 3 *Walker Street, Edinburgh, and all business communications, including Subscriptions, Advertisements, &c., to* THE SUB-EDITOR, Dr NORMAN WALKER, 7 *Manor Place, Edinburgh.*

THE SCOTTISH
MEDICAL AND SURGICAL
JOURNAL

ON THE INCREASE OF DISEASES OF THE NERVOUS SYSTEM AND OF INSANITY[1]

By WILLIAM W. IRELAND, M.D. Edin.

AMONGST the physicians who have treated of the increasing nervousness of our time, none has been more prominent than Professor Erb, of Heidelberg. The last published discourse of this eminent neurologist on this topic may be read in the *Revue Scientifique*, 2nd April 1898. But if we are to overtake other parts of the subject, we cannot do more at present than to indicate the main points in Dr Erb's thesis. He dwells upon the increase of the causes of nervous irritation and depression, the greater anxiety, hurry, and mental toil of life, the over-tasking at school, and the increased amount of irksome study men have to go through in order to enter the learned professions, the more exciting nature of their amusements, the more stimulating character of their diet, and the restlessness introduced into their lives through the facilities of inter-communication. The causes being thus increased, the effects are bound to follow. For my part I should have liked if the learned professor had essayed to pursue his deductions a little further, and afforded some direct evidence of this increasing nervousness, which, he says, has been going on for the last thirty years. Most of us are aware of the difficulty of earning a living wage at the outset, and of keeping up against unceasing competition, and of the greater excitement and complexity of life. It may be said that this strain falls most severely upon the middle classes. The diffusion of education amongst all ranks, the

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struggles of aspiring persons amongst the poorer classes to rise in the world, the discontent and bitter wars of labour, entailing incessant disputes and frequent strikes and breaches of contract, the spread of the co-operative movement which is rendering the position of the private shopkeeper more difficult to hold, and the formation of big companies which crush out small businesses; all these, and some social antagonisms on which we have no time to dwell, go to make it a harder and less agreeable world to many.

At the same time the new conditions of our to-day life are not without their compensations. If we move about more, we obtain more frequent amusement and change of air; if letters and telegrams too often claim our attention and disturb our repose, they often banish uncertainty and anxiety. Sounder views on dietetics have become known, and drunkenness has become less prevalent; the decrease in intemperance in liquor amongst the middle and upper classes during the last half century has been very marked, and there is reason to believe that this decrease is extending to the lower classes. In general we lead more secure lives than our fathers did, live under better laws, and have less to fear from injustice and oppression. There can be no doubt that during the last half century the condition of the working classes has greatly improved, their wages are doubled, while the purchasing power of money has increased for most articles, and they live under much better sanitary conditions. The result has been a notable diminution of the death-rate both in England and in Scotland during the last quarter of the century. Nevertheless it appears to me that the strain upon the nervous system is getting greater, at least for the middle and upper classes, and that therefore we may expect more nervous derangement and breakdowns. But to make a general opinion like this evident by exact inquiries is, on the face of it, very difficult.

There are some statistics to answer the questions: Is insanity increasing? Is idiocy increasing? And is the number of suicides increasing? But the inquiry whether other forms of diseases of the nervous system have become more frequent can scarcely be approached in that way, as many such disorders do not affect the mortality returns or appear as results in statistical tables of any kind. Anxious to obtain the opinions

of physicians of experience and reputation on this question, I addressed to a number of medical men the following queries:

1. Have you found since you began to practise medicine that there has been an increase in the relative frequency of diseases of the nervous system?

2. Have new forms come into prominence?

3. Have you observed any change in the type of old diseases?

To these circulars I received a good many interesting answers, some of them from men whom I only knew by reputation. Several of my correspondents took the occasion to point out the dangers of fallacy in answering these questions. Medical men generally begin to practise amongst the poor, and in the course of time come to have more patients amongst the richer classes, who are subject to diseases of a different character. Some physicians gain in the estimation of the public a reputation for the treatment of specific maladies or diseases of different organs, and thus gain more patients of one kind than another. The most trustworthy answers towards our inquiry might be expected from medical men who had long practised in one district in the country, or in small towns, treating every class of patients alike; but these gentlemen who have done me the favour of answering my inquiries all declare that they see no reason to affirm that diseases of the nervous system have increased in their districts relatively to other diseases. Some physicians of experience practising in large towns are of the same opinion. Sir W. T. Gairdner and Dr MacVail in Glasgow, Dr George Balfour in Edinburgh, Dr Leech in Manchester, Dr David W. Finlay in Aberdeen, Dr Henry Barnes in Carlisle, and Dr Grieve, late Surgeon-General of British Guiana, have noticed no increase in the relative frequency of nervous diseases.

We may spare time for the reply of Dr A. M. M'Aldowie, of Stoke-on-Trent: "I should be inclined to say off-hand that nervous diseases are more frequent now than formerly; but on examining the facts, I feel convinced that the increase is apparent only, and is due to the fact that one recognises early system lesions, neurosthenia, etc., in much slighter forms than fifteen or twenty years ago. On comparing my case books (I keep a record of every case I see, in private or hospital), I

cannot find that diseases which were well recognised at the period I mention, *e.g.* epilepsy, chorea, locomotor-ataxy, are more frequent now than then. My own opinion, therefore, is that diseases of the nervous system are not on the increase."

Some physicians point out that there has been during the past twenty years a greater differentiation in the diagnosis of nervous disease, and that the nervous element in ordinary diseases is better recognised than it used to be.

On the other hand, some physicians of reputation in the same towns, but presumably treating a different class of patients, have stated their opinion that nervous diseases have increased in frequency during the last twenty years.

Some of the medical men in their replies nicely balance this and that consideration, and hesitate to pronounce a formal opinion. The only physician, save one, in the United States to whom I have written—Dr Osler, of Baltimore—is of this neutral class.

Amongst those who have given a categorical reply that they believe nervous diseases are on the increase, are Sir Frederic Bateman, of Norwich; Dr Eddison, Leeds; Dr James Goodhart, London; and Dr Ernest Reynolds, Manchester. Dr A. James has taken some trouble to further the inquiry, as may be seen from his letter: "I have been looking up the ward journals of the Infirmary, and I find in the late Professor Sanders' journals, 1869-1878, there were 144 nervous cases out of a total of 1519 male cases. In my own journals, 1892-1899, there were 155 nervous cases out of a total of 809 male cases. This gives a tremendous increase in the nervous cases. I thought there would be an increase, but I did not expect such a great one. Surely there must be some fallacy, but I cannot yet see where it comes in. I chose Sanders' journals because I knew them best, as I was a clinical clerk, and resident with him."

I also venture to add part of a letter from Dr Julius Althaus:

"1. I consider that the relative frequency of diseases of the nervous system has decidedly increased. This increase, however, holds only good for functional or nutritional diseases, and not for organic affections of the brain and spinal cord. Of these latter, those which are acknowledged to be owing to syphilis, have,

in my experience, decidedly diminished during the last twenty years.

"2. The functional diseases are now prominent over structional ones, but I cannot say that actually new forms have sprung up in our time, except perhaps the neuroses owing to influenza. Of these latter no, or next to no, mention is made in the older writings, but of course this does not prove that they have not occurred. It appears to me more likely that the old physicians have failed to see the connection between influenza as an exciting agent, and the nervous sequels of it. This would be analogous to the influence of syphilis. Hunter, and even Romberg, denied that syphilis ever caused lesions of the nervous centres, but it seems impossible to assume that in their time syphilis did not act in the same manner as it does now, in producing an immense variety of nervous affections."

As Dr C. H. Hughes, of St Louis, U.S.A., Dr David Drummond, of Newcastle, and Dr Urquhart, of Murray's Royal Asylum, Perth, like Dr Althaus, are well known as neurologists, their opinion that nervous diseases are much increasing may be held merely to signify that such patients are likely to drift to them. Dr George Keith is decidedly of opinion that nervous diseases are much increased since he began to treat disease almost sixty years ago. He attributes it in great part to the abundance of flesh meats now allowed to children. Dr Strachan lays much stress upon the evil effects of overtasking children at school, a subject to which he has given much attention. Most physicians who believe nervous diseases to have become more frequent, consider that the increase falls in the lighter forms, especially in neurasthenia.

Few physicians have noticed any change in the type of diseases. Dr Leech, who has noted none in nervous complaints, has observed a distinct difference between the rheumatic fever seen in earlier times than that seen now.

Dr Byrom Bramwell is inclined to think that there has been an increase in some of the functional neuroses, and he has definitely formed the opinions that disseminated sclerosis has, during the past few years, become commoner, and that this increased frequency is probably due to the fact that in many cases this sclerosis follows, or at least seems to have its starting point in influenza. He also thinks that the grandiose ideas in general

paralysis are now often wanting. But this, he thinks, may be because those cases in which such exalted ideas are not observed were not formerly classed under the heading of general paralysis.

Dr William M'Lachlan of Dumbarton has no doubt that diseases of the nervous system are increasing. He has observed an atonic irritative condition to be more common. The patients feel that they are on the verge of losing their mental balance. The causes of this are commercial competition, social aping at what the income must be strained to meet, stock exchange gambling to catch fortune by a few lucky deals. In the female, sexual indulgence and the using of measures to obviate pregnancy—uterine disappointment followed by motor and volitional disturbance. Acute sthenic diseases are not so common. Pneumonia and fevers are more of the adynamic type than when he first met them twenty-five years ago. Hysteria is not so common; tennis and the cycle are helping to give it a quietus.

This recalls the old question of a change of type in fevers and inflammations which was debated half a century ago between Doctors Allison and Christison on the one side, and Dr Hughes Bennett on the other.

Dr Halliday Douglas, long physician to the General Post-Office at Edinburgh, saw many cases of nervous derangements and failures of health amongst the employés, especially those who did their work under high pressure. The female clerks often required leave of absence from illness.

I have learned from several sources that monotonous work demanding severe and unremitting attention is most trying for the nerves and assists in causing mental derangement. Thus nervous diseases and insanity are especially common with telegraphists, letter-sorters, and persons who have to check mechanical labour.

From all the answers which I have received I should judge that there has been no noticeable increase in the number of diseases in the country districts and in small towns, but there is some evidence that there has been an increase in those complaints in large towns, especially in functional disorders such as neurasthenia. Probably this increase affects some classes, and persons of special pursuits more than others.

Graver Forms of Nervous Diseases

A physician interested in my inquiries who held that the increase was mostly in the lighter forms of nervous diseases was inclined to think that there might be fewer cases of the graver forms; but a study of the registers of mortality showed me that the contrary was the case. I spent several days, through the courtesy of Dr Blair Cunynghame, in the Register Office examining the statistics there. One thing appeared at the outset, that those diseases of the nervous system which are assigned as causes of death are less frequent in Scotland than in England. To quote the Registrar's report: While in Scotland in 1857 only 170 died of Brain and Nervous diseases in every hundred thousand, in England during the same year 273 deaths occurred in a like population from the same causes, and year after year the relative proportions remain somewhat the same in the two countries.

In Scotland the proportion of deaths from Diseases of the Brain and Nervous System for every hundred thousand of the population was—

	Total for Scotland	Males	Females	In Towns			Mainland Rural	Insular Districts
1855	168	191	148	213			148	97
1856	161	179	146	203			143	81
1857	170	192	151	223			144	94
1858	169	192	148	211			148	104
Average of ten years from 1855 to 1864	170 both sexes			214			147	97
1865	179	199	161	233			149	113
				Principal Towns*	Large Towns	Small Towns		
1875 †	217	239	198	245	285	208	185	96
1885 †	231	248	214	264	252	226	209	95
1895	226	233	219	237	249	224	206	125
1896	206	211	201	215	221	206	190	144
1897	221	233	209	234	231	220	199	173

The general death-rate for Scotland was in 1855, 20·6 in the thousand; in 1897 it was 18·8 in the thousand. That is,

* This is a new sub-division of the Registrar.

† At Table XXVIII., of Report XXVIII.

to come closer, in Scotland there died of brain and nervous diseases in 1855, 168 persons to every hundred thousand of the population; in 1865, 179 persons; in 1875,217 persons; 1885, 231 persons; and in 1897, 221 persons. The increase from 1855 thus amounting to 53 persons, about one-third of the first number. The proportion of deaths from different diseases in every ten thousand persons from diseases of the nervous system was—in 1855, 818; in 1875, 930; in 1885, 1210; and in 1897, 1109. In the report for 1897 we are told that from diseases of the brain and nervous system there were 9257 deaths, giving a mortality rate for Scotland of 221 in every hundred thousand, and forming 11·79 per cent. of the total deaths. The rate for Scotland is exceeded by the principal and large town districts; the lowest fall in the insular rural districts. Among principal towns the highest rate falls in Perth, 330; in Paisley, 288; in Coatbridge, 262; in Greenock, 250; in Glasgow, 238; in Edinburgh and in Aberdeen, 232. The nervous diseases specified are apoplexy, paralysis, epilepsy, and convulsions.

One can only guess at the efficacy of the causes of nervous disease in the gross. Drunkenness should be a potent cause; but there has been noted a decrease in the number of deaths from delirium tremens and chronic alcoholism in the last twenty-five years. There was also an increase in deaths from diseases of the urinary organs,[1] which were doubled from 1855 to 1897. Diseases of the circulation had also doubled during the same period. The mortality from phthisis had fallen from 261 in 1855 to 172 in 1897.

I did not make the same careful examination of the vital statistics of England, as this has already been done in the book of Dr Newholme on that subject, published last year. He shows that while there is a great improvement in public health as indicated by a lower death-rate; commencing in 1872 it fell from 22·6 in the thousand down to 17·4 in 1897. Dr Newholme says that "it is doubtful if the increased strain of modern life exists in the community as a whole. Assuming, however,

[1] Urinary diseases, which comprise Bright's disease, acute nephritis, calculus, and affections of the bladder and prostate, stood in 1855 at 24, and in 1897 at 49 to the hundred thousand. During the same time circulatory diseases had risen from 78 to 170.

that over-pressure exists in certain stations of life, *e.g.* among city merchants, medical men, etc., it cannot be said generally to exist among professional men; clergymen, lawyers, and civil servants are as classes long lived." But clergymen and civil servants generally lead easy lives, and it is likely that anxiety and over-pressure may be felt less by the lawyers than by their clients. "Even assuming," goes on Dr Newholme, "that over-pressure exists throughout the whole of the professional classes, these do not form the mass of the community. The majority of the population of England and Wales belong to the wage-earning classes, and the condition of these classes will therefore necessarily have the greatest influence on the total result." It is easy to prove that the working classes are now in a more prosperous condition than they were half a century ago. Newholme, however, shows that there is a high death-rate from diseases of the nervous system amongst barristers and solicitors, railway stokers, and medical practitioners. Diabetes, a disease connected with disorders of the nervous system, has steadily increased, as also renal diseases, which are high amongst doctors, lawyers, and innkeepers. Diseases of the nervous system show a death-rate per million of 1546 in 1861-65, and of 1600 in 1891-5. He observes: "In 1861-65 the mean death-rate registered as caused by intemperance was 42 per million. It steadily increased in 1891-96, reaching 68 per million. This increase is almost certainly due to more correct certification of deaths." At the same time the death-rate ascribed to cirrhosis, which I take to be generally the result of intemperance in drink, has increased amongst males from 43 in 1861-70 to 140 in 1881-90. There is also an increase in the death-rates from premature births and congenital defects.

Dr S. A. K. Strahan, in his book upon "Suicide and Insanity" (London, 1893), calls attention to the fact that, "while the general death-rate for England and Wales has fallen 16·4 per cent. during the past quarter of a century, a rise, in some cases amounting to over 100 per cent., has taken place in the death-rates from hereditary and degenerate diseases."

From a table compiled by Dr Strahan, it appears that the death-rate in England and Wales in the twenty-five years be-

tween 1866 and 1890 had increased for nervous diseases by 10·2 per cent.; from diabetes, 103; from kidney disease, 63·4; and from heart disease, 65·9.

From these statistics it seems to me that it is not going in advance of the evidence to pronounce that the graver forms of diseases of the nervous system have increased in Great Britain, as they are said to have done in France and Germany, during the last forty-five years. In Scotland there seems to have been a stoppage in the increase from 1885 to 1897, which, I hope, indicates that the rise of the death-rate from these maladies is not fatally progressive.

Suicide

Since the publication in 1879 of Morselli's work, "Il Suicidio," the attention of sociologists has been directed to the portentous increase of suicide in Europe and North America. This increase is most marked in great cities, and seems to be steadily progressing in every country in Europe save Norway. It is needless here to repeat what I have written in my paper on "The Causes of the Increase of Suicide" in the *Journal of Medical Science* for October last.

On this subject Dr Strahan observes: "Taking the recorded numbers as being relatively accurate, we find that in the twenty-two years, 1867-88, the number of suicides has risen steadily. In the former year there were 1316 discovered suicides in England and Wales, and in the latter year 2308, which gives an increase of over 75 per cent. in the twenty-two years. That increase of population is not responsible for this difference is shown by the fact that, while the rate was only 61 to the million persons living in 1867, it had attained 82 to the million in 1888, an increase of more than a third within the twenty-two years.

"This steady and rapid increase of self-destruction is common to the whole civilised world. It is most marked in those countries which take leading parts in the world's doings, but it is noticeable in all. Dr D. R. Dewey has recently been studying the question in the New England States of America. He finds that since 1860 suicide has increased

about 35 per cent. In Massachusetts it has increased in thirty years (1860-90) from 69·9 to 90·9 to the million living, and in Connecticut from 60·6 to 103·3 per million."[1]

In Scotland the rise from 1865 to the quinquennial period of 1890-94 has been stated to be from a rate of 40 to a rate of 54, an increase of 35 per cent. From my own inquiries I have found that in 1881 there were 182 suicides in Scotland—131 males and 51 females—out of a population of 3,735,573, giving a proportion of 48·7 to the million. During the quinquennial period within the years 1881-85 the mean annual number of suicides was 202, equal to 52·05 to the million. In the quinquennium, 1886-90, the mean number of suicides was 227, giving 56·4 per million. In the quinquennium, 1891-96, the mean number of suicides was 247, giving a proportion of 59·2 to the million. In the year 1896 there were 297 suicides—216 males and 81 females. This gives a proportion of 71·2 to the million.

As the causes which tend to produce suicide are of the same character as those which tend to produce insanity, greater sensibility and irritability, and a lessened power of reaction, it is not surprising that those who have committed themselves by denying that insanity is increasing should be ready to question any rise in the number of suicides. Their position is incongruous, for as they advance that the increase in the number of registered lunatics is owing to the increased number of admissions of early and milder cases of mental derangement, they have to escape the awkward question: Why, since many of these cases are of a pronounced suicidal character, is there not a decrease instead of an increase in the gross number of suicides? As the statistics in both questions are against them, they have to show that the statistics which indicate an increase of suicides are "crude," and then to show that the uncooked statistics which reveal an increase of lunatics should be looked upon with disfavour. Apparently they will be content with a verdict of "not proven."

It seemed to me that enough had been said about Dr Sibbald's doubts of the increase of suicides in Britain in considering "The Causes of the Increase of Suicide" in the *Journal of Medical Science* and in the debate thereon; yet since Dr

[1] "Suicide and Insanity," p. 186.

Clouston gave his opinion that I should not have passed this by, and Sir John Sibbald has repeated his views before a learned society, of which I am not a member, I venture to add a few words. He lays great stress upon the fact that suicides by hanging have not increased in England and Scotland during the last thirty years, while self-destruction by other methods is reported as increasing. There seems no reason for presuming that the means used for suicide should always keep the same proportion. It is known that in the fashion of making away with themselves men are much influenced by imitation, and during the last thirty years hangings have been much rarer and no longer a public spectacle. Practically death by strangulation has been displaced by dislocation of the spinal cord. With the spread of information easier and less repulsive methods of death are likely to be selected. Less conservative people, like the native-born Americans, have mostly given up hanging for self-destruction, hence we need not be perplexed that it in these islands lags behind the general increase of suicides.[1]

There has been no change of any consequence in the manner of inquiring into sudden deaths in Scotland. Self-destruction is still regarded as a deplorable event in the family; but there has been no opposition to burial in the churchyard for many long years. While some suicides are put down as being drowned or being killed by accident, few accidental deaths are registered as suicides. Altogether, though there are no doubt stray errors and omissions in the registers, these are not sufficient to vitiate the general results.

Idiocy

The statistics of idiocy taken from the census are untrustworthy, owing to the reluctance of parents to admit that their children are so affected. Wildermuth has observed that the

[1] I have found that in Scotland during the five years between 1881-85 the mean yearly number of suicides by hanging was 57, giving a proportion of 14·6 to the million. In the quinquennium of 1886-1890 the mean number of such deaths by hanging was 66·8, equal to 16·6 to the million. In the quinquennium, 1891-1896, the mean number of suicides by hanging was 66·8, giving a proportion of 16·01 to the million. Thus even suicides by hanging had increased during the last two quinquennial periods over the first. In the year 1896, out of 297 suicides, 69 were by hanging, giving a proportion of 16·5 to the million.

number of idiots is decreasing in Würtemberg; but we think this only indicates a diminution in the cases of endemic cretinism, which is everywhere declining in number owing mainly to the people being taught to avoid drinking water from sources which are known to cause goitre and cretinism. In my book "On Idiocy and Imbecility," published twenty-three years ago, statistics were given showing that there had been a fall in the number of idiots in Norway between 1855 and 1865 of 14·63 per cent. Unfortunately this has not been sustained. In 1865 there was one idiot in every 834, and one insane in every 524 inhabitants; but by the census of 1876 there was one idiot in every 498, and one insane person in every 398. It was hoped at first that this unpleasant result might be owing to the questions being put in so indefinite a manner that mistakes were made in the answers. In the census of 1891, which was carefully taken, there was one idiot for every 823 inhabitants, and one insane person for every 376. In Sweden there was found to be, in 1860, one insane person to every 771 of the population; in 1890 there was one lunatic to every 550. In 1860 there was one idiot to every 1542 persons, in 1890 one to every 628. These statistics thus bring out an increase both in idiocy and insanity in Sweden and in Norway, although we have been assured that drunkenness has much diminished in these two countries owing to the working of the Gothenburg system.

By the last census the number of idiots in Ireland was stated to have fallen from 8639 in 1881 to 6243; in 1881 there was one idiot to every 598 of the population; in 1891 one to every 754. The number of lunatics had increased from 9774 to 14,945.

Increase of Insanity in England

The steady and progressive increase of the number of the insane throughout Europe has now begun to attract public attention. Much of this increase is due to collection of lunatics in special asylums, and to their accumulation, owing to better care giving some of them a longer life; but this fails to explain the whole rise, and physicians like Koch, Kollmann, Cettingen, Lunier, and Kraepelin, who have devoted time and

care to examine the statistics have, after making every allowance, come to the conclusion that there has been a real increase in the absolute number of the insane. Unhappily the Commissioners in Lunacy for England took a different view, and as the number of lunatics they had to deal with perversely kept on increasing, they have had much care to explain away their own statistics. There were always, however, some men well acquainted with the subject to show that there was a real increase, Sir James Crichton Browne amongst them, and the phenomenal rises in the number of "official lunatics" in 1897 and 1898 have opened many eyes. In 1897 there was an increase of 2919 registered lunatics in England and Wales, and in the next year an increase of no fewer than 3114, the largest annual rise which the Commissioners have had to record. In 1859 there was one official lunatic to every 536 persons. In 1869 there was one to every 418. In 1879 one to every 363. In 1889 there was one registered lunatic to every 337 persons, and in 1898 it was one in 302. For the last ten years there has been a yearly increase of over 2000 lunatics.

The inference that this denotes a real increase is confirmed by the returns of the last census. In 1871 there was stated to be one insane person to every 329 of the population; in 1881 there was one insane to every 307, and in 1891 one to every 298.

The following is taken from the *Journal of Mental Science*, April 1899. "Dr Bowes has issued a most interesting report on the existing high rate of lunacy in Yorkshire. The first question discussed is whether the apparent increase of insanity in England is real or only due to accumulation of the chronic insane and the removal of many quiet dements from their homes. Dr Bowes agrees with the general impression that there is some increase, but not enough to create alarm. He points out that there is a much larger proportion of insane to sane among the agricultural labourers than among miners and city dwellers. He shows that for thirty years the agricultural counties have shown most pauperism and most insanity. He thinks that marrying in and the poorness of the living and generally depressing vital conditions account for this. All this is true, but we believe that it is true also that the more feeble are left in

the country to breed degenerates and to live from hand to mouth."

A considerable increase in pauper lunatics has also been noticed in the counties of Argyleshire and Inverness-shire, where all the more energetic inhabitants betake themselves to other places for employment. Nevertheless there may be exciting causes of insanity acting even in the rural districts in this unquiet age. We have seen that the graver forms of nervous diseases are increasing in the insular districts of Scotland.

Dr Hastrup, a Danish physician,[1] has noted that new nervous diseases have appeared even amongst the Esquimaux, which some believe to be owing to the great use of coffee, which the Esquimaux obtain from the traders in exchange for skins.

Insanity in Scotland

The same steady increase in the number of the insane goes on in Scotland as in England, and there has been the same startling rise in 1897 and in 1898 which has distanced all arithmetical explanations. In 1898 there was an increase of registered lunatics of 487, of whom 49 were private patients and 438 were pauper patients. At the beginning of the present year the figures furnished by the General Board of Lunacy gives since 1858 up to 1898 a net increase of 9575 in the number of lunatics under the jurisdiction of the Scottish Lunacy Board, or 64 per cent. The increase of the population has been only 41 per cent. The census returns for Scotland showed in 1871 one insane person for every 494 of the population; in 1881 one insane for every 444; and in 1891 one insane for every 385.

The author of an able communication which appeared in the *Scotsman* on the "Growth of Insanity in Scotland," who has evidently great command of information, writes that the average increase may be put down as at 2·4 per cent. yearly, or more than double that of the population increment; but where the position is adjusted by deducting the deaths, removals, and recoveries, averaging 1·2 per cent. per annum, there is still this balance of over 1 per cent. to explain the

[1] Hospitals Tidende of Copenhagen for August 11 and 18, 1886.

accumulating process, which accounts for the swelling totals. The author of this communication further observes: "There is not much to justify the expectation of a higher recovery rate under present conditions. For quinquennial ending 1879 it stood at 16·5 per cent. on the average number on the register; in 1884 it fell to 15·5; in 1889 to 13·3; and in 1894 to 13·4."

The death-rate is much the same as it was in 1858. In some asylums which have many admissions from a city population it is even higher. Dr Clouston has remarked in his annual report for the year 1898 that the death-rate in Morningside within the first twelve months after admission has been steadily rising from 46·4 per cent. in 1874-78 up to its present rate of 51. It has been advanced by those who combat the belief that there is a real increase in insanity in Scotland that there is little increase in the number of private patients, and that the rise is mainly in pauper lunatics; but when one looks at the real naked facts he sees that it could scarcely be otherwise. During the last forty years the smaller asylums which took in those who could pay moderate boards have been squeezed out of existence. The few private asylums remaining are obliged to charge much higher, and the general expenses for the upkeep of the insane have much risen. For many years back it has been the custom that those bearing the burden of a lunatic relation who are anxious to pay what they can, go to the inspectors of poor to gain admission into the district asylums. The patient is certified to be a pauper lunatic and his board in whole or in part is paid by the relatives to the inspector of poor, who transmits it to the treasurer of the asylum. Thus a considerable number of those who are what would formerly have been private patients are now officially returned as pauper lunatics, although they are not really so. This probably leads to looseness of administration, and no doubt makes it easier for people to get rid of the burden of supporting a lunatic dependent than formerly.

Insanity in Ireland

The general report of the Census Commissioners shows that the number of lunatics in Ireland returned in 1891 was

nearly treble the number in 1851, and that the increase between 1881 and 1891 was from 9774 to 14,945, while there was a decrease of 9·1 per cent. in the general population. The population of Ireland in 1851 was 6,552,385; in 1881, 5,174,836; and in 1891, 4,704,750. From the Eighth Report of the Inspectors of Lunacy in Ireland, it appears that the total increase during the year 1898 was 714; it was larger than that for 1897, viz., 624, and exceeded the average increase for the past ten years, viz., 444. In the Report for 1897 the inspectors said that the rise was mainly the result of accumulation brought out by a low death-rate and a diminished number of discharges. They considered the number of admissions to asylums a more trustworthy source of information. In fact the admissions were less that year (1897), and in Ireland the admissions do not increase every year, but when we take the sum of every two years we find that they are progressively larger. Unfortunately for the inspectors' argumentation the admissions rose in 1897 higher than they had ever been.

Of the causes assigned for this increase we are willing to allow something to the accumulation of chronic cases in asylums, the greater readiness to grant admission to all cases of mental failure, aided by the four shillings grant first given from the Imperial Exchequer twenty-five years ago to ease the local rates. Much stress is laid upon the greater willingness of the friends of lunatics to send them into asylums owing to the increase of confidence and the skilful and humane way in which lunatic asylums are now managed. This, of course, is an argument most pleasing to the superintendents of these institutions. It is capable of indefinite extension. A sudden leap upwards in the admissions only means that the public confidence in asylum stock had risen. I am far from insinuating that this increased confidence or lessened distrust is not what it should be, but I am doubtful of its extent and intensity. It is but ten years ago since, owing to a popular outcry of suspicion, the superintendents in England were shackled with a new Lunacy Act, bristling with galling restrictions and insulting returns. Few people know anything about asylums till some of their relations go mad, and then they get them out of the house simply because they have become intolerable in

it. What keeps them back from doing so is mostly the expense, which is assuredly not diminishing, for patients whose friends can pay. Moreover, the probability of heredity of insanity or idiocy is now getting diffused, and has begun to render people wary of making a damaging admission by sending their kindred into institutions which are becoming ever more public. While granting something to all these explanations, it is the outcome of my inquiries that they fail to account for the whole increase of insanity. Again and again have these purveyors of explanations been obliged to make their premises bigger to account for the swelling numbers of the last few years. They remind me of a man who has pronounced that the sea tide was not rising, and who persists in imagining that every falling back wave was bigger than the incoming one, while all the while he is again and again driven to change his position by the flowing tide which represents facts and not arguments.

There are men in the country who have the spirit to look at things as they are, and these I should ask to recognise that we have to deal with an advance along the whole line, an increase in nervous diseases, in suicides, and in lunacy.

After this paper was finished I took up the last edition of "Psychiatrie,"[1] by Dr Emil Kraepelin, where I found a complete agreement with my views. I translate one passage: "On the other hand, regular census with us indicates with certainty a rapid increase of the insane which far overtops the general increase of the population. No doubt this rise is partly accounted for by the greater care of the numeration and the better knowledge of insanity. Nevertheless, I believe we cannot any more doubt that we have actually to reckon with a very considerable increase of insanity. This is proved, not only by the alarming increase in the number of the insane, but also by the simultaneous rise in the frequency of suicides, and the contrast presented by the town and rural populations."

The learned professor observes that we are living in a time of transition, and he hopes that coming generations will be able to enter on the stress and struggle of life with fresh strength and better weapons. We need not believe that this

[1] "Psychiatrie ein Lehrbuch für Studirende und Aertzte." Sechste Auflage. Leipzig, 1899. 1 Band, p. 88.

increase of diseases of the nervous system is destined to fatal progression; it may stop or it may retrocede ere long. It would be rash to assume that we are acquainted with all the causes, and the means of prevention are not entirely beyond our control.

DELIVERY BY BASILYSIS[1]

By A. R. SIMPSON, M.D., F.R.C.P.Ed.,
Professor of Midwifery in the University of Edinburgh

IMPROVEMENTS in the construction of the Forceps and a better appreciation of the conditions under which Turning, Cæsarian section, and Symphysiotomy can be safely carried out, have in recent years greatly restricted the group of cases in which the obstetrician feels called upon to deliver by Cephalic Embryotomy. Some have even proposed to relegate the whole store of perforators, comminutors, and extractors of the head to the presses of antiquarian museums. That there is still, however, a not inconsiderable field for the employment of embryotomy procedures, may be gathered from the circumstance that at least six of the numbers of last year's *Centralblatt für Gynækologie*, vol. xxiii., contain one or more papers on the subject. If the cases are rare, there is all the more need that the practitioner have in his hands the implements with which he may most simply and safely bring about the head-reduction and delivery.

In 1880 I read a communication to this Society, in which I proposed the use of the Basilyst for first perforating the cranial vault, and afterwards perforating and dissolving the cranial base. In 1882, and again in 1883, I related cases illustrative of the value of the operation, and showed a head that had been delivered by basilysis (see Fig. 1). I explained further that, to fit the instrument to act as an extractor as well as a perforator and comminutor, I had added a traction blade to be applied outside the head, like the external blade of a cranioclast. Abundant evidence had been adduced from many quarters in proof of the superiority of the cranioclast to the cephalotribe as an extractor. The chief drawback to the cranioclast is that in some cases it does not fracture enough of the base of the

[1] Read before the Obstetrical Society, Edinburgh.

skull to allow of the perfectly easy extraction of the head. It was to supplement this deficiency that I proposed the basilyst, with the further idea that in many cases such dissolution of the basic structures would be produced as to allow of the expulsion of the head by the natural efforts.

Since that time various suggestions have been made for the improvement of the cranioclast, such as that of Dr Hubert Peters (*Centralblatt für Gynækologie*, xxiii. 472, 1899), who would have the internal blade furnished with a point that

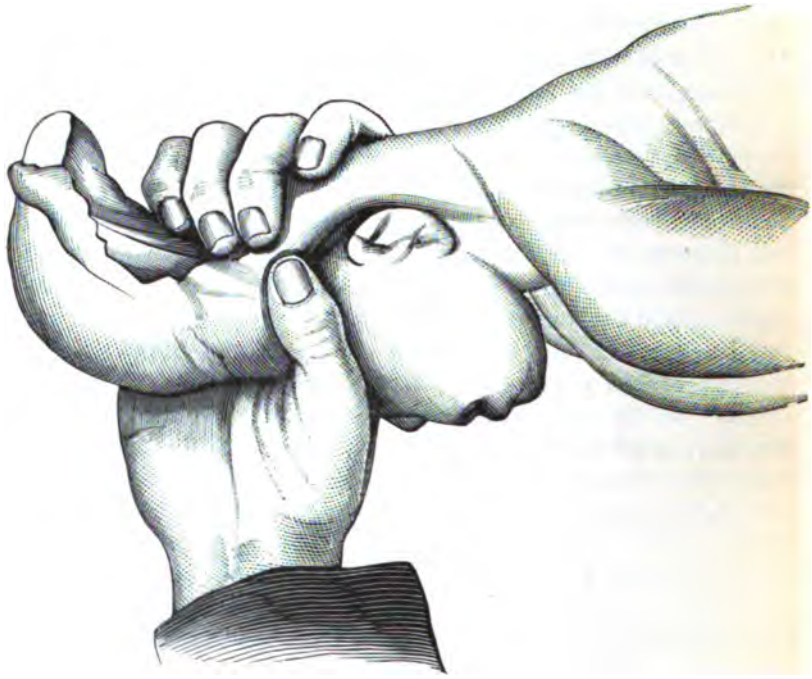


FIG. 1.

should allow of its being screwed into the foramen magnum, or some part of the base, so as to get a more secure hold for traction, as well as to allow of fuller crushing of the base than can be accomplished with the ordinary cranioclast. A still greater number of three-bladed contrivances have been proposed, which should have the object of adding to the cranioclast the power of the cephalotribe for compression of the head. These are exemplified in Auvard's Embryotome Cephalique Combinée

(*Arch. de Tocologie*, xvi., 1889), Zweifel's Kranio-Cephaloclast (*Centralblatt für Gynækologie*, xxi. 482, 1899), Fehling's Kephalo-thrypt-helktor (*Centralblatt für Gynækologie*, xxii. 1177, 1898). That which has been most approved is the Basiotribe of Professor Tarnier, which he produced in 1883, and the value of which has been demonstrated in the essay of M. Bonnaire (*Sur le Broiement de la Tête Fœtale*, Paris, 1885), or more fully and with numerous illustrations in the work (*De l'Embryotomie Cephalique*, Paris, 1889) of M. Paul

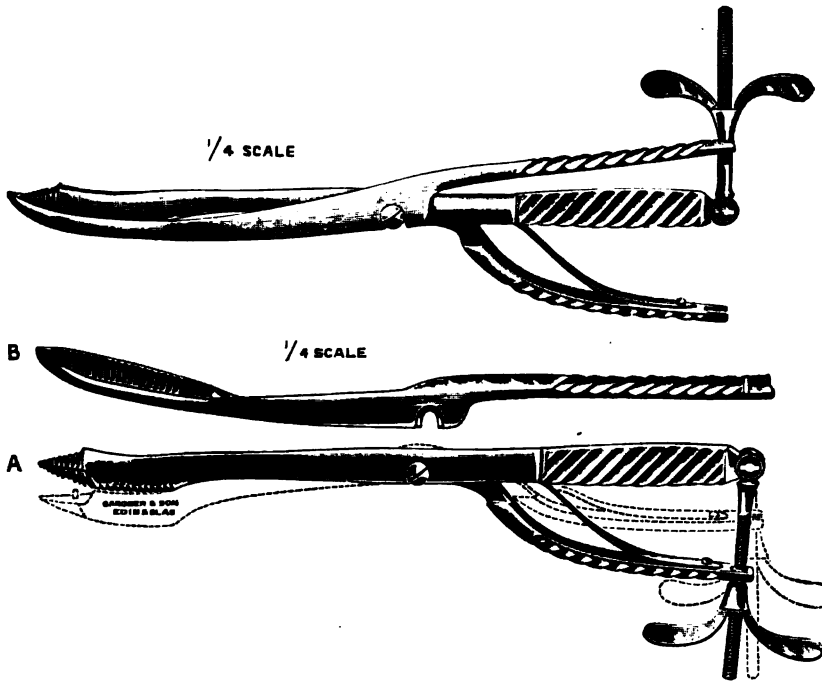


FIG. 2.

Bar, one of the ablest of the brilliant group who loved and looked up to the great Parisian obstetrician as their "Maître." The basiotribe consists of three parts—(1) a straight perforator that is to pass through the vault and on to the base; (2) a first blade to be applied outside the head and jointed to the perforator, so as to make of it a modified cranioclast; (3) a second blade that is to be applied when necessary to the side of the head opposed to that which was caught by the first, so as to

make of the instrument a modified cephalotribe. The record of cases of basiotripsy shows that in the hands of Tarnier and his successors excellent results have been obtained by the use of his instrument. But I cannot get rid of the conviction that the first effort of the obstetrician should be to break up the base from within, and not from the outside. The application of the external blade is not always very easy. It often implies the previous introducing of the hand into the genital cavity, with danger of laceration of the uterus and access of septic germs. All this is increased when two blades have to be carried into the uterine cavity outside the fœtal head. And although the basiotribe is an effective comminutor, the flattening of the head between two blades cannot admit of the elongation and adaptation of the head to the configuration of the canals, which is such a favourable feature in the extractions effected by the cranioclast. Moreover, I am more and more convinced that in many cases the basis cranii can be so simply and safely and effectively dissolved from within the skull as to render quite unnecessary the application of even one external blade. As an illustration, let me give you the history of a patient who was recently delivered by Basilysis in the Maternity Hospital. I show you the head of the infant, after it has been hardened in formaline and cut in the coronal direction from above downwards. The base has been perforated in its anterior portion, and has been so fractured and softened that between the points of the calipers in front of the ears it measured 1½ inches in width (see Fig. 3).

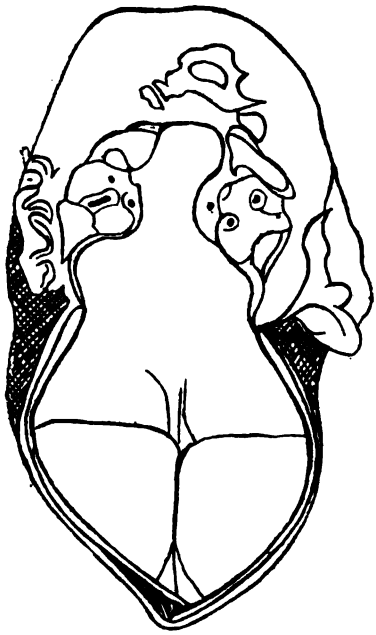


FIG. 3.

E. F., æt. 18, stunted and dwarf-like in her appearance, was admitted into the Maternity Hospital in the ninth month of her first pregnancy, on the 2nd January 1900. Patient was 4 ft. 4 in. in height, and was the subject of well-marked right dorsal scoliosis and left lumbar scoliosis. The right leg measured, from the anterior superior iliac spine to the external malleolus, 26½ inches. The left measured, between corresponding points, 25¼ inches. The right thigh was 13¼ inches in length; the left, 11¾ inches. There was marked tilting of the pelvis, the left side being highest. There was no evidence of any previous hip-joint disease. Patient was sent into hospital because a serious difficulty was expected at the impending labour owing to the pelvic deformity.

Her mother stated that patient was unable to walk till she was five years old, but says that up to her thirteenth year she noticed nothing definitely wrong in her configuration. At the age of thirteen she took scarlet fever, and it was after she came out of hospital that, for the first time, it was noticed that the back was "crooked." Since this illness patient had not grown in height, and the spinal deformities had gradually become more marked. Patient's general health has been good.

Physical examination. — Breasts well developed; gland tissue abundant, with plentiful secretion; nipples prominent; areolæ well marked.

Abdomen.—Protuberant; abdominal swelling corresponding in size to periods of amenorrhœa passed. (Patient had menstruated last in April 1899, and stirrage was first felt in August.) The swelling lies somewhat obliquely with long axis from the right hypochondrium to the left iliac region. Abdominal walls lax, linea nigra and striæ gravidarum well marked. The uterus is directed obliquely downwards from right to left, with the fundus at the lower border of the ensiform cartilage. Fœtal head felt low down above the brim. Limbs on the right side. Uterine bruit and fœtal heart sounds both audible.

Vaginal examination.—Sacral promontory is abnormally prominent and somewhat deviated to the right side. Cervix is high up and to left side of promontory. Fœtal head felt presenting above the pelvic brim, freely movable.

Pelvic measurements.—Inter-cristal diameter, 10¾ in.; inter-spinous diameter, 10 in.; external conjugate, 6⅞ in.; diagonal

conjugate, 3½ in.; conjugata vera, 3 in. A universally and unequally contracted pelvis.

Slight and irregular pains began at 3.30 P.M. on the 19th of January. The os was fully dilated at 6.15 P.M. The liquor amnii began to escape at 6.45 P.M. The presentation was vertex, the position L.O.A. Drs Berry Hart, Freeland Barbour, and Fordyce saw her along with me. We discussed the alternative procedures that might possibly have been adopted for effecting delivery—turning, forceps, symphysiotomy, Cæsarean section. The girl was a pallid, delicate creature, who appeared to us a bad subject for the graver forms of interference; and at 8 o'clock, the head being still loose above the brim and projecting over the symphysis pubes, I delivered by means of the basilyst, Dr Barbour having his hand applied to the head of the child in the hypogastric region.

Waiving any discussion as to the relative indication in this particular case, what is specially important to have in view is the ease and safety of the whole procedure. With the basilyst the vault was perforated and an opening made wide enough to allow of the complete breaking up of the brain and cerebellum, which were washed out with a lysol douche. The head not descending, the same instrument was screwed into the anterior part of the base and the blades separated in different directions. I was prepared further to basilyse the head, and had the traction blade at hand if it had been required. But, I confess to my own surprise, when I was about to repeat the attack at another part of the base, Dr Barbour who had his hand on the uterus above the brim said, "Wait, the head is going down." After a few pains, the uterus, aided by the pressure of Dr Barbour's hands, had brought the head to the pelvic floor, so that with my finger through the opening in the vault it was easily guided on its further course till it was clear of the canals. After the delivery was completed, we made careful examination of the canals and saw that there was only a slight fissuring of the mucous membrane in the vaginal orifice at the base of the hymen. The patient had an uninterruptedly normal convalescence.

I fancy the basilyst would have found wider acceptance if in the earliest models I had not made the mistake of getting too heavy an implement. Experimenting on heads of infants

that had lain some time in pickle, it was necessary to have very powerful instruments to tear up the bones. But since I found that the bones of the fœtus *in utero* are more easily dilacerable, I have had the basilyst (see Fig. 2) made slighter and more manageable. Further, the instrument maker at first had the traction blade so imperfectly adapted to the internal portion that it sometimes slipped. In the newer model this defect is remedied. The internal perforating and external grasping portion fit like the two blades of a cranioclast, so that for extraction, when that becomes necessary, the basilyst-tractor has the qualities of what is admittedly the best instrument for the delivery of the perforated head.

RECRUDESCENCE IN SCARLATINA

With details of a Case in which the Temperature was higher in the Axilla than in the Mouth

By WILLIAM TAYLOR, M.D., F.R.C.P.Ed.,
Edinburgh

IN 1775 Benjamin Franklin, who was a sufferer from gout, wrote as follows: "I find I grow daily more feeble. . . . Some symptoms of the gout now appear, which makes me think my indisposition has been a smothered fit of that disorder, which my constitution wanted strength to form completely."

Which my constitution wanted strength to form completely! We are accustomed to think that as strength recedes disease advances; that therefore a weakly person must succumb to an acute disease, whereas a robust person shakes it off as a vigorous bird does its loose feathers. But it is not always so.

How often do we see the typically strong man suddenly cut down, whereas the weaklings pass through a lifetime of suffering, yet never die—until they have earned the certificate that "age and debility" formed the chief factors in the condition constituting the cause of death.

Do acute diseases then require a certain amount and variety of *pabulum* before their individuality can be fully developed and sustained?

Is this the reason why, in some patients, only certain traits of a disease can be discerned, from which we have to gather its

identity, feeling sure that the other symptoms ought to be present, but they are not?

If viewed subjectively as a disease, it must be considered imperfect. It is not a well-developed, full-blown specimen of its class. Is this the reason why certain diseases, when assuming the mild form, as it is called, are followed by deadly sequelæ, which, properly speaking, it was no part of their function to generate?

Take scarlatina. Why are mild cases followed by swollen glands, deafness, nephritis, and other ills? And why, although we anticipate these and use every possible means to arrest them, are we unsuccessful? These ailments form no part of the disease proper, or they would accompany it in every case. They are not necessarily the result of cold or carelessness. Why do they only or chiefly supervene on mild or imperfect attacks, and seldom or never on the severer examples?

Let us return to gout. Sydenham says: "The more violent the inflammation and pain the shorter will be the paroxysm and the longer the period of subsequent immunity."

Is this because the system has acted in *harmony* with the disease by presenting the necessary conditions for its vigorous manifestation, "*the strength wanted to form it completely*," and because, in virtue of this, it has been possessed of the vital powers necessary for ensuring a subsequent compensatory reaction? Such is the course of regular gout as met with in well-nourished patients.

As a contrast, take the tedious, troublesome, "poor man's gout," the atonic or retrocedent form which occurs where the system is wanting in these conditions.

Cullen says: "A second case of variation in the course of the gout is when, to the atony, the reaction and inflammation have to a certain degree succeeded; but from causes, either internal or external, the tone of the extremities, and perhaps of the whole system, is weakened, so that the inflammatory state, before it had proceeded to the degree requisite for restoring the tone of the system, suddenly and entirely ceases. Hence the stomach and other internal parts relapse into the state of atony, and perhaps have this increased by the atony communicated from the extremities, all which appears in what we have called the *retrocedent gout*."

Although analogies are necessarily defective and often fallacious, does it not seem reasonable in the light of past experience and the teaching of the present day, that disease should be viewed as a plant, bearing seeds; that these may fall upon good ground and bring forth abundantly; that they may fall on unsuitable soil and not germinate at all; and that they may fall on imperfect soil and spring up partially, their roots invading the neighbouring territory, thereby damaging *it* without deriving from it the proper nourishment of the plant, which therefore withers, but remains latent until fresh soil is added in the form of renewed strength in the patient, and the plant springs up again more vigorous than before.

This seems to bear some resemblance to the conditions accompanying what is called *recrudescence.*

The following case of scarlatina simplex may in some measure serve to illustrate this suggestion—which I have not quite the courage to formulate as a theory, but which I am contented to adopt as such until I find a better.

Eliza, aged twelve, a weakly, growing, active girl, living at home and attending school. Badly nourished, not for *want* of nourishment, but on account of the unreasonable conditions attending school life in Edinburgh, which subjects her and many others to a daily routine something like the following:—A hurried breakfast at 8.30; a scrap "nosebag" lunch at 12; tea consumed voraciously at 4; abundant family dinner just played with at 7; thereafter lessons, or recreation, or both, till bedtime. The great omission here is what every pupil ought to have—a diet of fresh air and recreation from 12 to 1, and a good rest and dinner before resuming work at 2.

E. stood at the top of certain of her classes, and took life seriously. The daily output of energy exceeded the input of food. Therefore she was badly nourished.

On October 31st she caught cold, experienced a slight rigor, and remained indoors. I saw her on the following evening, and found her throat slightly inflamed. Temperature 102°, pulse 123. No other symptoms, save just the suspicion of a rash. I suggested the possibility of it turning out to be a mild scarlatina, when I was met with the exclamation from the

anxious parents, "And shall we have to keep her from school for six weeks, and must the others lose their schooling for the same period?" Such cases followed, as this was, by a fall of temperature, are very perplexing. They do not afford sufficient data to warrant you in forming a decided opinion for your own guidance, and in conveying your impressions to others it is not easy to steer evenly between the enjoining of necessary caution and arousing unnecessary apprehension; and it is especially difficult to parry the storm of interrogation, bordering on reproach, which is apt to be hurled at you for the very suggestion of scarlatina.

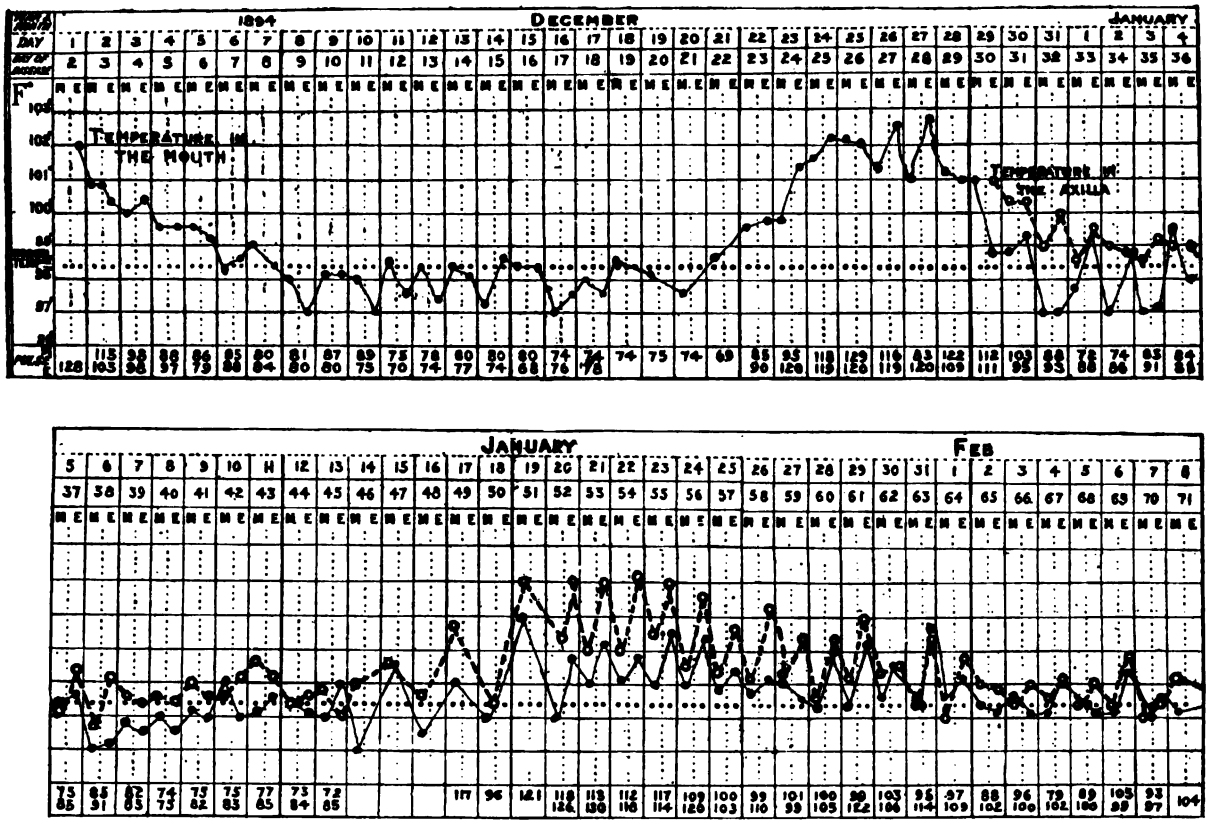
The subsequent course of the trouble will be found delineated on the accompanying chart, where it will be seen that the illness was divided into three distinct periods, each extending to from three to four weeks.

During the first few days of observation the temperature kept falling steadily towards normal, which it reached on the seventh day. Thereafter it kept oscillating between normal and subnormal until the end of the third week. "The inflammatory state, before it had proceeded to the degree requisite for restoring the system, suddenly and entirely ceased."

With the fourth week the first period of recrudescence set in. The temperature rose suddenly, and remained so for seven days. Stimulated by the rest and nourishment, the reaction occurred which previously the "*constitution wanted strength to form completely.*"

On the eighth day the temperature fell, and remained varying from 97° to 99° for twenty-one days. During this time the system lay low and underwent the process of recuperation so effectually that at the beginning of the eighth week it was able to set up a second and final recrudescence and to sustain it moderately for two weeks, after which convalescence set in and health was speedily re-established.

During the first recrudescence I was struck with the seemingly high temperature of the skin, and having shaken down the mercury I placed the thermometer in the axilla, and was surprised to find that my sense of touch had not deceived me. This practice was repeated daily, with the same precautions. The same thermometer was kept for the same length of time



TEMPERATURE CHART OF DR TAYLOR'S CASE.

THE DOTTED LINE IS THE TEMPERATURE IN THE AXILLA, THE PLAIN LINE THE TEMPERATURE IN THE MOUTH.

in the axilla and in the mouth on forty-nine separate days, and on thirty-three of these the temperature of the skin was higher than that of the mucous surfaces underneath the tongue. On referring to the accompanying chart it will be found that the heat of the skin, as indicated by the dotted markings, was two degrees higher in the morning and three degrees higher in the evening of the thirty-second day of the disease than it was in the mouth, whilst on twenty-one days it was from one degree to two degrees higher. On sixteen days the skin was cooler, ranging between normal and subnormal, sometimes below, but never above, the temperature of the mouth. These variations seemed to follow no rule.

I do not know whether other observers have noted the same conditions. They are entirely new to me. I have looked for them in many ordinary cases, but have not yet found another example.

The rash, as I have already indicated, was very faint at first, so that I had to ask the patient's mother whether the skin was its natural colour or slightly reddened, and she could hardly give a positive reply. But during the first recrudescence the rash was very copious and widespread. It then faded and reappeared less copiously during the second recrudescence. The sore throat did not return. Desquamation began early but meagrely, until after the first recrudescence, when it became abundant. It continued for nine weeks, and finally the nurse became impatient, and I found her one day rubbing the soles of the feet with pumice stone.

The urine was tested daily. Albumin appeared in it three times. It followed each exacerbation. During its continuance pure milk diet was allowed and *nothing* else. Immediately on its cessation each time a liberal and varied bed-diet was given, including stewed and sometimes raw fruit, the latter sparingly.

During the period of the third appearance of albumin the glands on the left side of the neck became enlarged. These have been more or less affected ever since, but have not suppurated.

Throughout the whole illness the patient had the advantage of a skilled and capable nurse, and her surroundings were all that could be desired.

Three years after.—The swelling of the glands just alluded to followed closely upon the removal of flannel round the neck, where it had been employed in anticipation of, and as a preventative of, glandular irritation. It was, I fear, too soon dispensed with, but if so it was speedily re-applied and long retained. The swelling was at first very amenable to treatment and sometimes entirely disappeared; but the changeable weather of an inclement spring brought it more into prominence, and at length after many months of variation the swelling became less diffuse, and a knotted chain of three or four hardened glands evidently undergoing caseous degeneration remained. These were so temptingly near the surface, so callous and indifferent to treatment, and so menacing in their attitude to neighbouring glands, that it was thought wise to remove them, as well as several others more deeply seated which the operation brought into view. The wound healed by first intention, and now hardly a trace remains of it.

Three of the superficial glands were caseous; the fourth was tuberculous. The deeper glands had not reached the caseous stage.

Do we find in this tuberculous gland the key which reveals that lowered condition of system which the rest, warmth, feeding, and nursing were unable to restore, and which prevented the disease from acquiring its full development? And was the determination towards the skin, as manifested by increased temperature, the result of a vigorous endeavour to avert a threatened inflammatory invasion of the deeper glandular economy? And is it possible that we in our eagerness to obtain a speedy recovery, or by some other good-natured indiscretion, may sometimes hinder, or even thwart the working out of the delicate details involved in that beneficent design?

THE CHRONOLOGY OF ANTE-NATAL LIFE

Read before the Edinburgh Obstetrical Society, March 14th, 1900. With two Schedules

By J. W. BALLANTYNE, M.D., F.R.C.P.Ed., F.R.S.Ed.,
Lecturer on Midwifery, Medical College for Women, Edinburgh; University Lecturer on Ante-natal Pathology and Teratology, Edinburgh, etc.

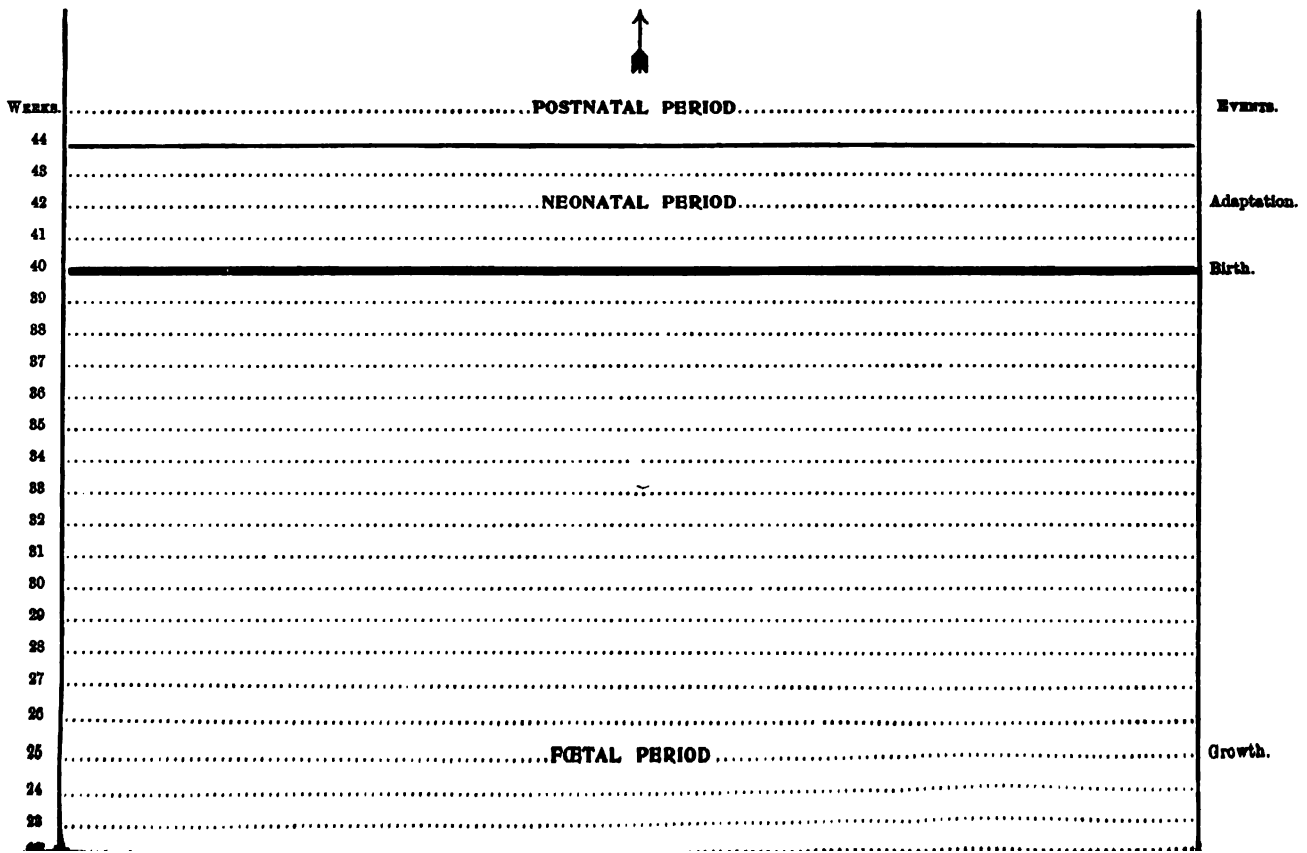
IT is probably no exaggeration to say that few obstetricians have a very clear perception of the progress of events, normal and pathological, during ante-natal life. The drama of embryonic and fœtal development and growth is going on, but the curtain has not been rung up, and the spectators get only confused impressions from the swaying of the drop-scene and from vague sounds, "excursions and alarms," coming from behind it; yet no one doubts the existence of great activity, *post cortinam theatri*, and some from superior knowledge can even judge how preparations are progressing; further, the bill of the play gives an inkling of what is to be soon displayed with great spectacular effect. The two schemes which I am about to describe have been constructed in order to give to the mind a somewhat clearer conception of the chronology of the period of preparation for the great events of post-natal life. That they are far from perfect, I hasten to admit; that they may, nevertheless, be of some use, I venture to claim.

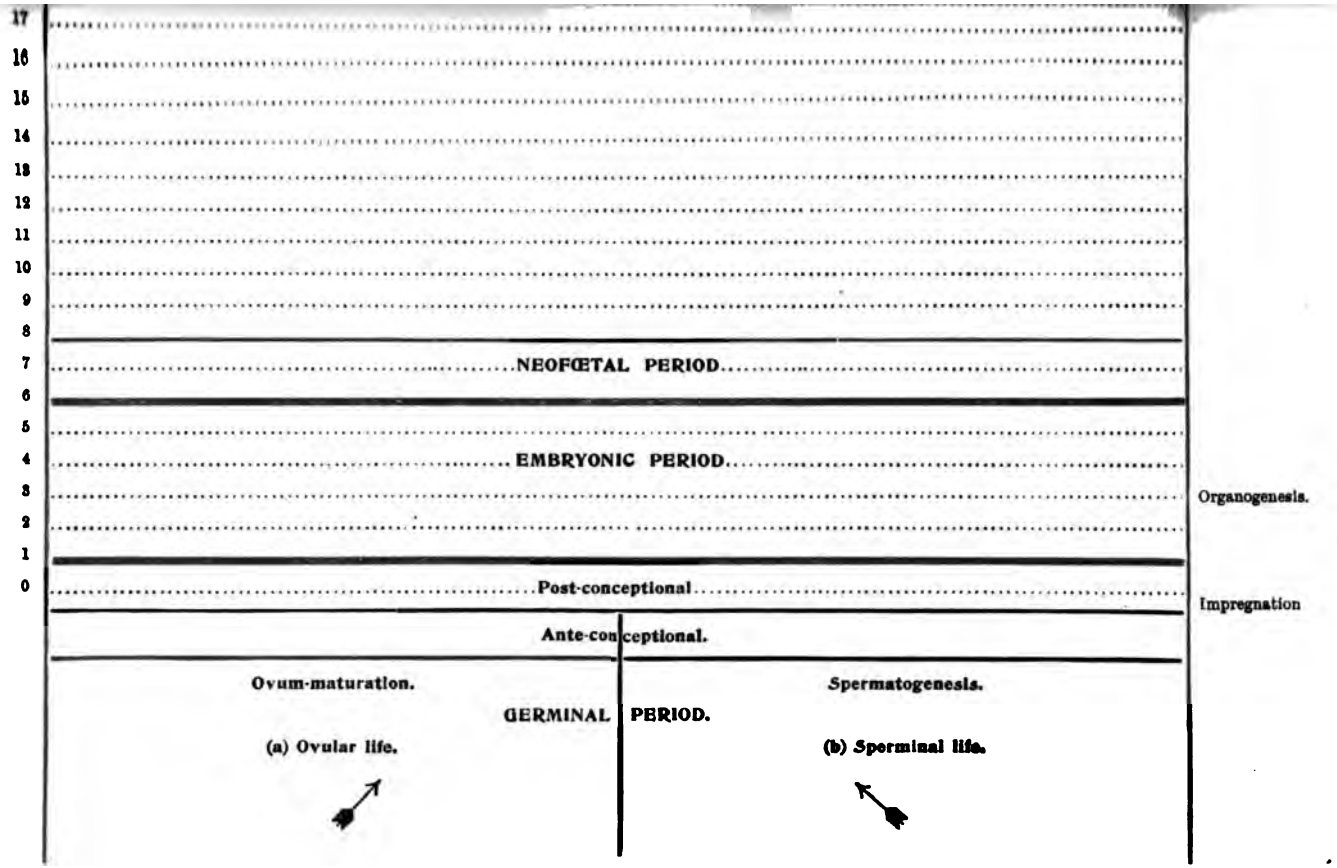
In constructing the scheme of the divisions of ante-natal life (Schedule I.) I have employed the "space-for-time" method introduced into medical case-taking by Mr Jonathan Hutchinson. By this plan, all periods of time are represented in the schedule by equal extents of space, no time is left out, and the whole duration of the ante-natal epoch, with its various events in their proper places, is brought correctly before the eye. Mr Hutchinson has written of the value of the method in ordinary case-taking (*Arch. Surg.*, vii. 199, 1896), and I think it is not less in the recording of ante-natal phenomena, both normal and pathological. The divisions of ante-natal and of early post-natal life, as shown in the schedule, are grouped in the manner that I have been accustomed to arrange them in my various contributions to the literature of ante-natal pathology during the past four or five years.

Each interspace in the scheme represents a week; and as pregnancy lasts normally for forty weeks, there are forty interspaces intervening between its beginning and end; but as the month following birth is much influenced by what has happened before birth, and is, indeed, a transition period between ante-natal and post-natal life, it also has found a place in the scheme, and has four interspaces. Again it may be supposed that impregnation happens a few days after insemination, so half an interspace or half a week has been added on at the other end of the table; and below this is the rest of germinal life, which may be said, in one sense, to be without beginning. The thick black lines indicate the divisions between the germinal and the embryonic periods, between the embryonic and the fœtal, and between ante-natal and neo-natal life; the thinner black lines mark off the sub-divisions of germinal life and the close of neo-fœtal and of neo-natal life. At the left side of the schedule are numbers representing the weeks of which intra-uterine existence is made up, and at the right side are named a few of the great events, such as impregnation, organogenesis, growth, birth, and adaptation.

The earliest period of ante-natal life is the germinal, and only a small part of it at the close comes into the epoch of gestation. It has a long, a very long primary dual period, during which an independent life of a cellular kind is going on in the male and female reproductive cells, the ovum and the spermatozoon. The close of this dual or ante-conceptional period is marked in the case of the ovum by the phase of maturation, and in that of the sperm by the probably analogous phenomena of spermatogenesis; then follow the dehiscence of the ovisac and the passage of the mature ovum into the Fallopian tube, and the deposition of the spermatozoa in the vagina; and so ovular and sperminal life may be said to run together in impregnation, which may be regarded as occupying the intra-conceptional period of germinal life. The rest of germinal life is the unified post-conceptional period during which the morula mass and blastodermic vesicle are forming, and the first traces of the embryo appearing in the embryonic area. I have attempted to represent some of these facts in the scheme; the dividing line indicates the primary dual character of germinal life, and its absence in the post-

THE DIVISIONS OF ANTENATAL LIFE.—SCHEDULE I.




17
16
15
14
13
12
11
10
9
8
7
NEOFŒTAL PERIOD
6
5
4
EMBRYONIC PERIOD
3
2
1
0
Post-conceptional
Organogenesis.
Impregnation
Ante-conceptional.
Ovum-maturation.
Spermatogenesis.
GERMINAL PERIOD.
(a) Ovular life.
(b) Sperminal life.

conceptional period signifies its replacement by the unified epoch, while half an interspace has been given to impregnation to allow of the well-known fact that insemination and impregnation are not of necessity simultaneous.

The end of the first week of pregnancy may be said roughly to mark the commencement of the second or embryonic period of ante-natal life, which is anatomically demarcated by the laying down of the first rudiments of the embryo in the embryonic area of the blastodermic vesicle. This period has a duration of about five weeks, when it passes not abruptly but gradually into a sort of transition time, the neo-fœtal period, which intervenes between it and the distinctively fœtal epoch. The outstanding phenomenon of this embryonic period is the laying down of the scaffolding of the future body; its vitality shows itself in organ-formation or organogenesis; it has been thought by some that these organs are not functionally active till quite the end of this period, but of one at least of them this can scarcely be affirmed, viz., the heart.

The fœtal period of ante-natal life occupies by far the largest part of pregnancy; including the neo-fœtal or transition period it extends from the sixth to the fortieth week, or thirty-four weeks in all. It is marked by a marvellous growth and by the full establishment of the functions of ante-natal or intra-uterine existence. The most important event at its commencement is the full formation of the placenta, and its end is the transition from an intra-uterine to an extra-uterine life, viz., birth. Thereafter follows the neo-natal time, when the infant is undergoing certain changes to adapt it to its new environment, when the placental economy is being replaced by the pulmonary respiration and by the gastric digestion. Thus the forty-four weeks are accounted for. Other well-known phenomena might have been printed in the schedule, such as the time of maternal consciousness of the fœtal movements and of medical detection of the fœtal heart sounds; but it was not my intention to fill up the whole scheme in this way, but rather to leave space for observations.

With regard to pathological events, there are two times when death of the unborn is most likely to occur: in the neo-fœtal period (sometimes called on this account, "critical period"), and at or immediately after birth. At both these

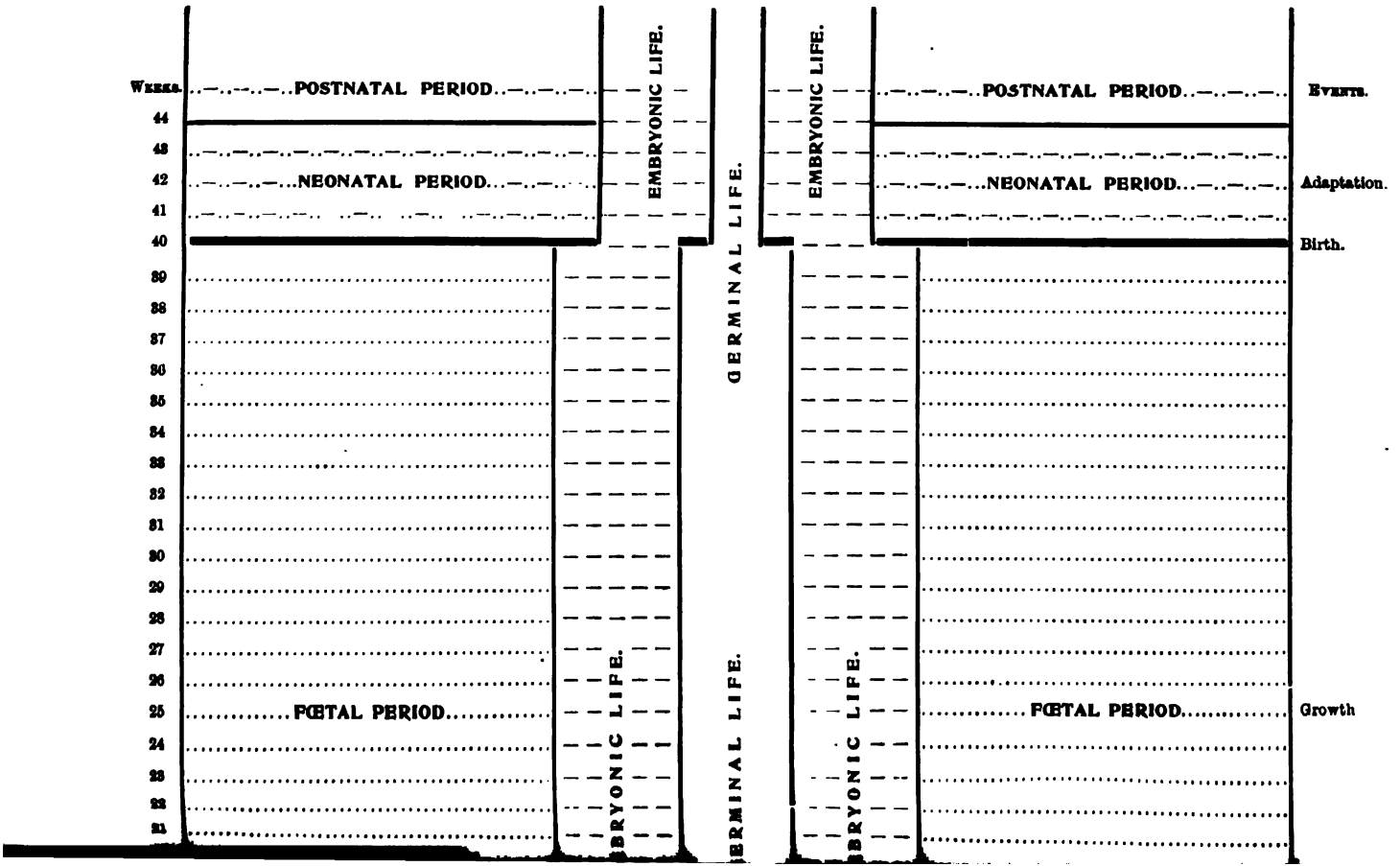
times there is a kind of readjustment in physiological conditions, with in consequence a predisposition to failure in the process. Short of actual intra-uterine death there may be in the fœtal period the development of various diseases, some of which are transmitted to the unborn from the mother, while others seem to arise idiopathically in it. In the embryonic epoch the result of the action of pathological agencies would appear to be the production of malformations and monstrosities of the monosomatous variety; and in the germinal epoch it may well be supposed that double terata, hydatid moles, included fœtuses, and blastoderms elliptically deformed and without embryos, are brought into existence. I merely refer to these peculiarities of ante-natal pathology here, they are discussed more in detail in contributions made during 1899.[1]

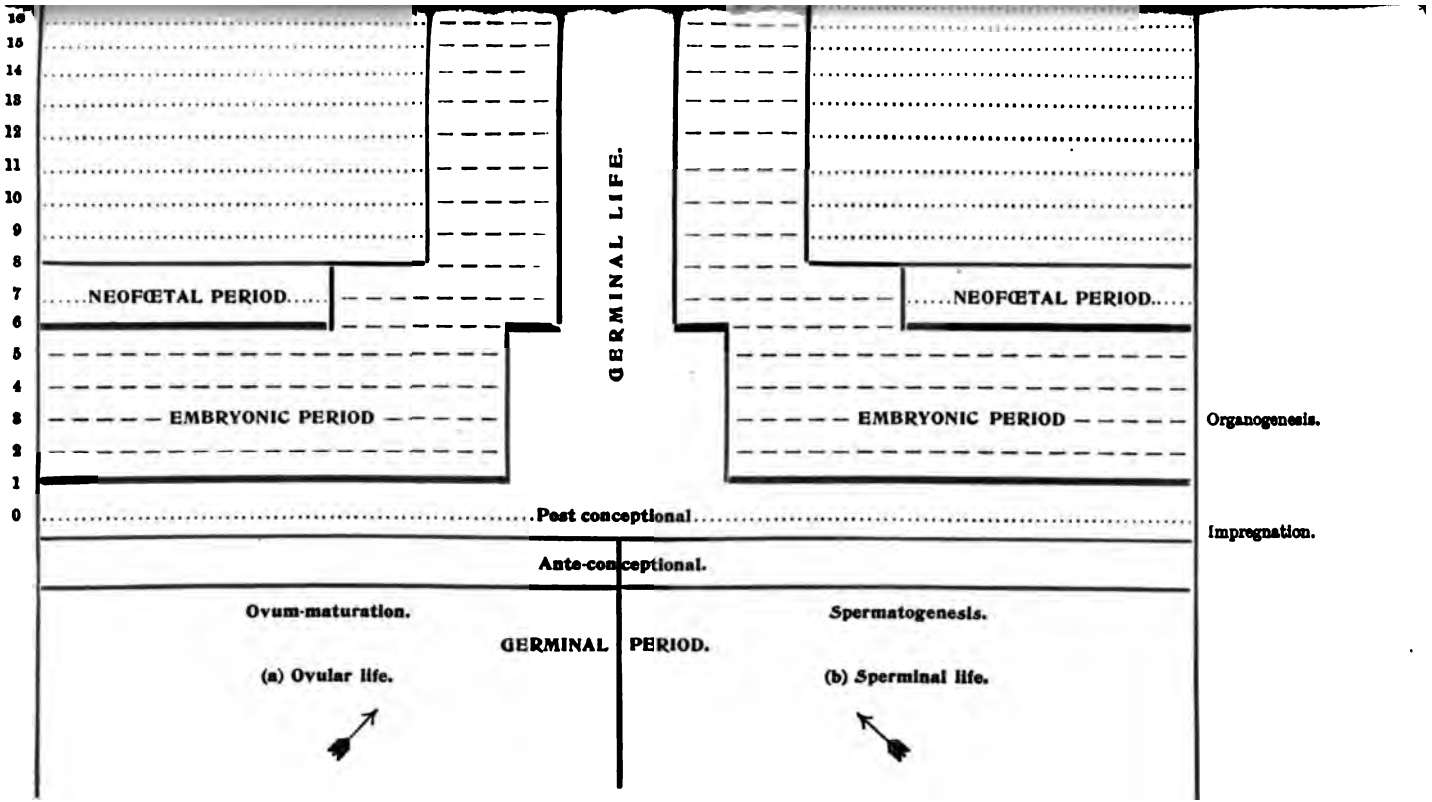
The schedule may be used for various purposes. It may serve to give the student of the phenomena of ante-natal life a clearer conception of the relative length of the divisions of this existence before birth and of the leading events which take place in it. It may also be of value as a case-taking card for midwifery cases, in which events, normal or abnormal, may be noted in their proper chronological order, and in which such observations as the examination of the urine for albumen with its result, positive or negative, may be inserted. It is impossible to predict in what exact ways the use of the scheme may recommend itself under varying circumstances, but in nearly every case of pregnancy that I have under my care at present, it has shown a new value. It is quite a simple matter to write in events and observations as they occur, and the number of weeks at the left side may be easily replaced by the exact dates if desired. When it is decided to induce premature labour for pelvic deformity or other cause, the weekly measurements of the uterus can be inserted in their proper places and the rate of growth checked and the date of intervention fixed with greater exactness.

The second schedule (Schedule II.) gives what may be called the corrected scheme of ante-natal life. It will probably have already struck the reader that the division of ante-natal exist-

[1] Ballantyne, J. W., "Pathology of the Fœtus," *Scott. Med. and Surg. Jour.*, v. 112, 1899; "Pathology of the Embryo," *Ibid.*, v. 481, 1899; "Ante-natal and Intra-natal Factors in Neo-natal Pathology," *Jour. Amer. Med. Assoc.*, xxxiii. 1245, 1899.

SCHEME OF ANTENATAL LIFE.—SCHEDULE II.




16
15
14
13
12
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5
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2
1
0
GERMINAL LIFE.
NEOFŒTAL PERIOD.....
NEOFŒTAL PERIOD.....
EMBRYONIC PERIOD
EMBRYONIC PERIOD
Organogenesis.
Post conceptional.....
Impregnation.
Ante-conceptional.
Ovum-maturation.
Spermatogenesis.
GERMINAL PERIOD.
(a) Ovular life.
(b) Sperminal life.

ence by hard and fast lines into germinal, embryonic, and fœtal periods is not free from error. It is quite evident, for instance, that all the laying down of scaffoldings is not ended at the end of the sixth week, nor yet indeed at the end of the thirtieth week; all organogenesis does not take place in the embryonic period, but some of it goes on in the fœtal. In order to represent this fact graphically, I have carried a projection of embryonic life up through the neo-fœtal, fœtal, and neo-natal periods into the post-natal. The skeleton and the limbs are good examples of parts of the body whose embryology, so to speak, does not end with the embryonic epoch; the uterus is an instance of the projection of the embryonic still further onwards, *i.e.* into post-natal life. The germinal period also does not abruptly stop at the end of the first week of pregnancy, the character of abundant luxurious cell formation which so specially belongs to it is projected through the embryonic and fœtal periods and is seen in post-natal life normally in one organ, the reproductive gland, testicle or ovary. Abnormally it is evident in some forms of neo-plasms. The scheme also represents in a graphic form the continuity of the germ-plasm and of germinal life. Whether or no there is under certain abnormal circumstances a continuity of embryonic life must be left in the meantime uncertain; it is just possible that in post-natal life there may be occasionally a persistence of the power of forming not only tissues but actual organs, as is seen in ovarian and other dermoids. Normally, however, there can be no new embryonic organogenesis without a repetition of the phenomenon of impregnation. I might have made the scheme more complete if I had represented a projection of fœtal life into post-natal and more particularly into neo-natal life; but this would have overburdened it, and I have preferred to leave it in its present condition. At the same time there is a very real projection of this kind, and many of the phenomena of early infancy can only be explained by a reference to the preceding fœtal period of life.

It may be added that the schedule is of a size convenient for the type-writer, and those who prefer to print in the events by this means can therefore do so.

Sir Henry Littlejohn's Report on the Health of the City of Edinburgh, and Infection by Milk

THE Report of the Medical Officer of Health of the City of Edinburgh, which has just been published, is of much general interest, and contains comments upon topics which at present command considerable attention from the profession.

The population of Edinburgh, the Report informs us, at the middle of 1899 had increased by 3299 persons since the same date in the preceding year. It is satisfactory to learn that the tendency with this increasing population is to occupy dwellings in the suburbs, and to vacate the smallest and poorest houses in the most crowded parts of the town.

There was a slight rise in both the birth-rate and death-rate for the year.

The number of deaths from measles, scarlet fever and diphtheria were all lower than in 1898. Whooping-cough, however, accounted for a considerably larger number of deaths than usual, the death-rate for the year being ·64 per 1000, as against the average rate of ·44 per 1000 in the previous five years. Diarrhœa, as is usually the case, accounted for a large number of deaths, and Sir Henry thinks that the cases which were by many practitioners certified as enteritis were probably the same malady. He refers to Dr Newsholme's paper on the prevalence of diarrhœa in the large towns of the country, and points out that if diarrhœa and enteritis are classed together the death-rate would be much higher than that given by Dr Newsholme, who gave Edinburgh the place of having the fifth lowest death-rate among the thirty-one large towns in which he investigated the matter.

Sir Henry makes several complaints with regard to the terms used in death certificates. Under the heading of Diseases of the Circulatory System he says: "Of the total deaths attributed to this class, viz., 552, no fewer than 139 have been certified as due to Syncope. This is very unsatisfactory, since Syncope is often a mere symptom due to other definite and primary diseased conditions which ought to be stated in the death certificate. This is apparent when we consider that eleven of the so-called deaths from Syncope occurred in children under five years of age"; and then again, "Ill-Defined and Non-Specified Causes were stated in 204 instances, of which, as usual, Debility, Atrophy, Inanition account for the majority, viz., 184. It is to be hoped that such imperfect certification as is shown by deaths having to be

classed under the heading of Dropsy, Hæmorrhage, Tumour, Abscess, will soon disappear."

The notifications of infectious diseases numbered 5365, of which measles accounted for 3600.

A few cases of typhus fever occurred in 1899, being evidently a continuation of the epidemic of 1898. The last cases occurred in June 1899.

With reference to typhoid fever and diphtheria the Old and New Towns occupied exactly opposite positions. Typhoid was more prevalent in the Old Town and diphtheria in the New Town. This observation applies also to the year 1898.

Sir Henry's remarks on Milk as a Source of Infection are of so much importance and are characterised by so much shrewd common sense that we reproduce them in their entirety.

During the past year, I investigated several limited outbreaks of infectious disease where it was suspected that the infection was derived from specifically contaminated milk, and in four instances at least, I believe that the suspicion was well founded.

Fortunately, owing in great measure to the early knowledge we derive of any outbreak of disease through the Notification Act, and the prompt investigations and precautions we are thereby enabled to adopt in order to prevent further dissemination of infection, in no instance did the outbreaks extend beyond a limited number of cases. Before considering some points of interest in regard to the spread of infection by means of milk, I will briefly state the chief facts in connection with each of the four outbreaks, three of which relate to Scarlet Fever and one to Typhoid Fever.

Outbreak I.—Towards the end of February, during which month the amount of Scarlet Fever in the city was small, and the particular district in question was entirely free from the disease, the following cases of Scarlet Fever occurred.

Case.	Date of Notification.	Place.	Sex.	Age.
1	February 25	Douglas Crescent	M	5
2	„ 27	Eglinton Crescent	F	12
3	March 1	Eglinton Crescent	F	9
4	„ 7	Drumsheugh Gardens	F	15
5	„ 8	Haymarket Terrace	M	6
6	„ 11	Lansdowne Crescent	F	23
7	„ 13	Coates Gardens	M	20
8	„ 14	Coates Gardens	F	20
9	„ 14	Haymarket Terrace	F	3
10	„ 14	Borthwick Place	F	13
11	„ 15	Haymarket Terrace	F	7

Cases 2 and 3 occurred in one house, also cases 5, 9, and 11. One common link connected these cases, viz., they all obtained milk from one large dairy. On enquiry the dairy was found to be clean; the milk supplied to customers was obtained from the dairyman's own cows, being brought into town by vans from the dairy farm, which was situated close to Edinburgh. The farm was clean, the cows healthy, and the business generally conducted in a satisfactory manner. Every one connected with the farm and the dairy in town was well and healthy at the date of our examination on March 1st, but the fact was elicited that a girl who helped to milk the cows, and also attended in the shop during the day, had suffered from a sore throat about the middle of February. She consulted a doctor, who prescribed for her, but as her throat did not improve, she went home to reside with her parents in the country upon February 18th. Subsequent enquiries addressed to the doctor at her home resulted in the information that she had suffered from a sore throat, that she was then, March 2nd, quite well, and showed no signs of desquamation.

Although frequent examination was made of all connected with the business, no trace of illness could be detected in any of them.

The dairy supplied a large number of customers, but the above cases were the only instances of illness connected with the supply either in Edinburgh or in the district in which the farm was situated.

Outbreak II.—The following particulars relate to an outbreak of Typhoid Fever which occurred in the district of lower Morningside at the end of June and beginning of July.

Case.	Date of Notification.	Place.	Sex.	Age.
1	June 21	Jordan Lane	M	36
2	„ 22	Comiston Terrace	M	35
3	„ 22	„ „	F	32
4	„ 23	„ „	F	8
5	„ 29	Blackford Road	M	8
6	July 1	Jordan Lane	M	15
7	„ 1	Warrender Park Rd.	F	19
8	„ 3	Braid Avenue	M	18
9	„ 3	Jordan Lane	F	23
10	„ 3	„ „	M	25
11	„ 3	Miller Crescent	F	46
12	„ 6	Maxwell Street	F	15
13	„ 8	Jordan Lane	M	23
14	„ 8	Braid Avenue	F	11

Cases 2, 3, and 4 occurred in the same house, also cases 8 and 14; all the others were members of separate families living in different houses.

Typhoid Fever was, undoubtedly, more prevalent than usual in the city in the early part of last summer, but no cases had occurred in the district of Morningside during the four weeks previous to June 21st. Complaints were being made at this time in regard to a new sewer which was being constructed along a road in the neighbourhood, and which, it was asserted, gave rise to offensive smells. No cases of Typhoid, however, occurred amongst those living in the road in question, and it was situated some distance away from the localities affected with Typhoid Fever. A link in common between all the above cases was the milk supply, which was obtained from a dairy and cow-house (A) situated in one of the streets affected, and which was distributed directly from this source to all the cases except 5 and 7. These two were supplied by another milk-seller, who, however, bought a few gallons of milk every morning from A for sale amongst his customers. A case of Typhoid Fever (F., æt. 30) was notified on April 7th in the same house as cases 2, 3, and 4, and was supplied with milk from A. This case was not removed to Hospital, and it is possible that the subsequent cases in this house may have become infected from it.

The conditions existing at the dairy and cow-house A were extremely bad. The premises were very dirty, including the milk-house, cow-house, and yard, while behind the house there was a very offensive manure pit and privy. The young son of the dairyman was stated to have suffered from inflammation of the bowels about the middle of June, but I could not get any reliable corroboration of the statement, while the parents affirmed that the illness was slight, and was the result of an accidental fall.

The milk supply was not stopped, but the premises were thoroughly cleansed, and no further cases occurred after July 8th.

In this instance also the cases of illness represented only a small proportion of the customers supplied.

Outbreak III.—The next instance has to deal with Scarlet Fever, and is of a more complicated nature. It occurred at the commencement of September, and embraced 17 cases in the city.

Case.	Date of Notification.	Place.	Sex.	Age.
1	Sept. 1	Caledonian Crescent	M	5
2	„ 1	„ „	M	7
3	„ 1	Mansionhouse Road	F	15
4	„ 2	Caledonian Crescent	F	9
5	„ 2	„ „	M	7
6	„ 2	Warrender Pk. Road	M	23
7	„ 3	Caledonian Crescent	F	22
8	„ 4	Marchmont Road	F	18
9	„ 5	Grange Loan	F	30
10	„ 5	Melville Street	F	35
11	„ 5	Douglas Crescent	F	7
12	„ 5	„ „	F	6
13	„ 5	„ „	M	35
14	„ 5	Caledonian Place	F	5
15	„ 6	Caledonian Crescent	M	17
16	„ 6	„ „	F	5
17	„ 7	Douglas Crescent	F	30
18	„ 8	Caledonian Crescent	F	12

Cases 11, 12, 13, and 17 were in the same family. All the rest occurred in separate households. A case occurred on August 9th in Caledonian Crescent (a female, æt. 18), but I do not think that it had any connection with the above series, although the milk supply was the same as in case 1. During the fortnight preceding September 1st only 14 cases of Scarlet Fever were notified in the whole of Edinburgh, and none of these occurred in the neighbourhood of the streets subsequently affected by this outbreak. The first point which seems remarkable in the above list is the wide distribution throughout the city of the cases, and under ordinary circumstances one might well hesitate to associate them together. That there was a very close relationship will appear from what follows.

On August 28th the Medical Officer of Health for Midlothian reported to me that the daughter of a large dairy farmer at Gilmerton had been attacked with Scarlet Fever, and two days afterwards he reported that a maidservant in the same family had developed the disease. Both patients were removed at once to hospital, and the premises were disinfected. The farmer kept forty cows, and disposed of his milk, chiefly in Edinburgh, to no fewer than eight dairy shops, while he also supplied a large educational institution and an hotel with milk.

The dairy shops were situated as follows :—

2 in Marchmont Road.
2 in Caledonian Crescent.

1 in William Street.
1 in Buccleuch Street.
1 in Charles Street.
1 in Carnegie Street.

And they each received daily a supply of milk from the farmer in question, varying from one to ten gallons. This was not the only supply of these shops, in all instances milk was obtained from other sources and sold to customers. Cases of Scarlet Fever did not, however, occur in connection with all of these dairies, but, only in relation to five of them, viz. :—the two dairies in Caledonian Crescent giving rise to eight cases and one respectively; the dairy in William Street, five cases; both Marchmont Road dairies, three cases in one, and one patient in connection with the other dairy. No cases resulted from the shops in Buccleuch Street, Charles Street, and Carnegie Street, while no patients were reported from the Hotel or Institution supplied directly with milk. It is worthy of remark that all the three dairies which did not give rise to cases are situated in one district, and at a considerable distance away from the others.

The cases which occurred in Edinburgh represented, of course, only a very small proportion of the total consumers of milk supplied by the dairies in question, but it must be remembered—and it is a fact of very great importance in connection with milk-borne disease—that it is a very general custom for purveyors of milk to obtain their daily supply not from a single source, but from several, thus, of the purveyors mentioned above, some received milk daily from five different cow-keepers, and all from not less than two. This fact adds greatly to the difficulty of tracing the source of infection in epidemics due to milk, especially when it is remembered that the different farms supplying the milk are often situated in widely separated parts of the country, and that they are not under the direct control of the Sanitary Authority in which the milk is consumed. Another circumstance which is apt to obscure the source of infection is the fact that the various supplies to a dairy are delivered at different periods of the day, and hence the infected milk may sometimes be sold to the customer in the state in which it is received, while at other times it is diluted with milk received from a number of other sources.

It is by no means uncommon for a single farm to supply a dozen separate local purveyors of milk, having their shops situated in all quarters of the city, and hence disease may be disseminated broadcast, and in widely separated districts, from one infected source. The task of connecting the links of evidence together, under such circumstances, is a matter of considerable difficulty, and, necessarily, of time also.

Within one week after the removal of the second case from the farm, and the disinfection of the premises, the outbreak ended, no further cases being reported among consumers of the milk. In the list of cases, the date on which the notification was received is given, so that presuming that the first symptoms of illness commenced, in each case, two days prior to this date, the outbreak ended even more abruptly than the list appears to indicate.

Outbreak IV.—The last instance of milk-borne infection also relates to Scarlet Fever, and in many respects it is the most interesting and most conclusive in regard to the evidence of contamination, and the relationship between the disease and the consumption of milk. The number of cases involved was forty-two, and I give below a summary of them classified according to age and sex.

Sex.	Under 5 years.	5-10	10-15	15-20	20-40	40 and up.	Total.
Males . .	3	8	2	7	2	...	22
Females . .	3	1	6	6	2	2	20
	6	9	8	13	4	2	42

It is thus seen that twenty-three cases occurred under 15 years of age, and no fewer than nineteen above that age.

The distribution of the cases embraced a wide district in Morningside and Newington, and one case connected with the outbreak was reported from Portobello, a child having been removed there upon the appearance of illness in another member of the family residing in Edinburgh. The first notification was received upon December 23rd, and the last upon January 10th. After this date no further cases occurred in connection with the milk supply about to be referred to.

The facts in regard to the outbreak are as follows:—On December 25th we received information from Dr Clouston that four cases of Scarlet Fever had broken out at Craig House, and that he suspected that the infection was possibly due to the milk supply which was received from a large dairy farm in the neighbourhood. Two cases of Scarlet Fever, both children, one residing in Spylaw Road and the other in Blacket Place, had been notified to us during the two previous days, both of which received milk from the same dairy, though only one of them directly.

An investigation of the dairy premises and persons employed there elicited the following information. A young girl who was employed in milking the cows noticed on December 23rd that her face was peeling slightly, and she remembered having had a slight sore throat, and thought her face was swollen, about a week before. Otherwise, she complained of nothing, and had not been incapacitated from her work; in fact, she was found milking the cows on the date of our visit to the farm. A young man, who was also employed in connection with the dairy work, complained of a slight sore throat upon December 24th, from which he was quite better in two days. He suffered from no other symptoms, and in further proof of the undoubted mildness and indefiniteness of the symptoms in both these cases I may mention that they were removed upon December 27th to the Fever Hospital, and kept under close observation for several weeks, during which time no more definite appearances presented themselves.

No other persons connected with the premises showed any signs of illness, although a strict supervision was exercised over them. The farm itself was clean, the cows healthy, and the business generally appeared to be conducted in a satisfactory manner. There was a large number of cows, and the milk was sold directly by the dairyman to customers, with the exception, that he gave a daily supply to a sister who kept a milk shop and supplied customers in the East Newington district.

We have in this history certain definite data—viz., that no cases of Scarlet Fever existed in connection with this milk previous to December 23rd. On this date a girl employed in milking noticed some peeling on her face, and remembered having had a slight sore throat a week before. On the following day, December 24th, a young man engaged in the dairy work complained of a slight sore throat. These two persons were removed to Hospital on December 27th for observation, and the premises were thoroughly cleansed and disinfected. During the last week of December and the first ten days of January, forty-two cases of Scarlet Fever were notified among the consumers of the milk, after which date—although the milk supply was never stopped—the outbreak ceased as suddenly as it commenced. If the two suspected patients were the cause of the outbreak, and if their removal from the farm and the measures which were taken by the Sanitary Authority in regard to disinfection were satisfactory and prevented all further contamination of the milk, and if, further, the maximum period of incubation of Scarlet Fever is seven days, then no more cases of the disease should, theoretically, have developed after January 3rd. As a matter of fact only two did so, and these were

children, æt. 10 and 5 years respectively, in whom infection from another probable source was possible.

In the following table the cases are classified according to the date of notification and the date of appearance of the first symptoms of illness, from which it is apparent how erroneous it would be to rely upon the date of notification as an index to the date of incidence of the disease.

Date.	Cases Notified.	Number of Cases in which first symptoms appeared on this date.
Dec. 20	...	1
„ 21	...	2
„ 22	...	4
„ 23	1	5
„ 24	1	1
„ 25	5	7
„ 26	1	8
„ 27	4	...
„ 28	1	4
„ 29	8	2
„ 30	4	...
„ 31	...	2
Jan. 1	2	3
„ 2	2	...
„ 3	2	1
„ 4	3	1
„ 5	1	1
„ 6	2	...
„ 7	1	...
„ 8	1	...
„ 9	...	...
„ 10	3	...
Total . .	42	42

The above table clearly shows that the first day on which the milk produced its effect was December 20, three days before the girl referred to noticed her face peeling, but some days after she had complained of a sore throat; while, practically before we had any knowledge of the danger which had arisen, that is to say, on or before December 25, 20 persons had begun to show the first symptoms of the disease.

After the removal of the supposed sources of infection on December 27, 14 persons developed the disease, all with two exceptions, as previously stated, within one week of this date, and therefore, presumably, they received the infection from the milk on or prior to December 27th.

The above examples of outbreaks of disease in which milk has apparently played a leading part are typical of the difficult problems which a Medical Officer of Health has constantly to deal with in this relationship, and which entail upon him a grave responsibility.

There are amateur sanitarians on the subject of milk infection, who, whenever it could be shown that a few persons suffering from Scarlet Fever were getting their milk supply from the same source, would at once take it for granted that the milk was the cause of the disease, and stop the supply. Such procedure is no doubt easy to propound by irresponsible persons, and appeals to the sympathies of parents who have suffered from the disease in their families; but it is not so easy of execution by a Sanitary Authority, nor would it, in a large majority of instances, be either just or expedient.

It has to be remembered that milk supplies can only be stopped upon sufficient evidence being at hand that the milk in question is a source of danger to the Public, and by order of a Magistrate, and it is therefore necessary that this procedure should be exercised with judgment, if grave responsibilities are not to be thrust upon Sanitary Authorities, and injustice done to one who may be wholly innocent, and to whom the stopping of his business practically means ruin. Of the four instances which I have brought under your notice, I think that in two of them it would be exceedingly difficult to say with certainty that the milk was the cause of the outbreak. I refer to outbreaks I. and II. In neither of these instances was there any wide dissemination of the disease, and the total number of cases only represented a very small proportion of those who must have consumed the infected milk. In neither instance could we discover, after careful investigation, any authentic evidence of contamination of the milk, although there was a history of a person having been ill on the premises some time before, but in each case independent medical testimony negatived the presence of specific disease.

In my own mind I believe that the milk was the means of conveying infection in both instances; but it is one thing to have a personal opinion, and another to put in force legal powers to stop the milk supply in the absence of any stronger evidence.

In the two latter outbreaks we had sufficient evidence, no doubt, to entitle us to stop the supply of milk, but such a course did not appear to me necessary, as the suspected source of the contamination having been removed, and both premises having been thoroughly disinfected, only a continuance of the spread of the disease would, in my opinion, have justified stoppage of the

milk supply. This opinion was supported by the results in both instances, as, after allowing for those cases to develop, which one might expect to result within a week after the removal of the cause, no further spread of the outbreak took place.

The difficulty in the way of proving the existence of milk infection is greatly increased, in towns at any rate, from the custom of purveyors of milk obtaining their daily supply from several dairymen, as I have already mentioned. The result of this is that, in nearly all suspected milk infections, exhaustive enquiries have to be directed, not to one farm only, but to several, situated probably at considerable distance away, and widely separated from each other. Frequently it is only possible to obtain information as to the presence of any infectious disease at a farm through the local Medical Officer of Health, whose enquiries cause more or less delay, and may not be so exhaustive as the circumstances demand.

A guide of considerable importance to the determination of the question whether a given outbreak of Scarlet Fever is due to milk infection is, in my opinion, to be found in the proportion of persons above 15 years who are attacked, more especially if of the male sex. Scarlet Fever is a disease of childhood, a very large proportion of attacks occurring under 10 years of age. When, therefore, one finds a considerable number of adults suddenly affected with the disease, the conclusion is that there is some special cause to account for it. Nothing could be more striking than what occurred in the last outbreak I have mentioned, in which, on the same day, four adults, whose ages were respectively 22, 26, 29, and 41, were reported to have been seized with the disease while residing in one block of a large institution, previously free from Scarlet Fever, and constantly kept under medical supervision. The chances of four adults becoming infected by ordinary means, and all developing the disease on the same day, is very unlikely, but, on the other hand, the circumstance is quite consistent with a poisonous dose of the infection having been conveyed in milk.

If reference is made to the table showing the ages of those affected in this outbreak, it will be seen that 19 of the cases out of a total of 42 were persons above 15 years of age, a very remarkable fact, quite inconsistent with all experience of Scarlet Fever occurring as a result of dissemination by ordinary channels, but consistent with, and strongly presumptive of the disease being propagated by means of infected food.

There is one other point I should like to refer to, and that is the fact of how little we know of the conditions under which milk may become specifically infective. It is a common belief

that a person who is desquamating after Scarlet Fever is in the most dangerous condition for conveying the infection to milk, and yet I know of one instance where a woman living in a small cottage had four children, one on the breast, all profusely desquamating after attacks of Scarlet Fever; she milked her two cows twice daily, delivered the milk personally to her twenty or thirty customers, usually being accompanied in her rounds by one or more of her children, and yet not one case of Scarlet Fever occurred amongst any of her customers. Every Medical Officer of Health can supply examples of outbreaks of Scarlet Fever in country districts where other modes of infection could be excluded and where milk was, in all human probability the cause of the infection; and yet, on the one hand, there may only have been the mildest of sore throats, without any subsequent desquamation, in one of the persons at the farm, who need not necessarily have come in contact with the milk, or, on the other hand, the most careful enquiries may fail to find any evidence of illness existing or having existed on the premises, either in the cows or in those engaged in the business. Again, instances equally as striking as the one I have mentioned above occur, where undoubted Scarlet Fever of a severe type exists on a farm or in a dairy, without the disease being conveyed to the consumers of the milk.

The whole question of the conditions under which milk becomes infectious is surrounded with obscurity, and until fresh light is thrown upon it by future investigations, the duty of a Sanitary Authority must be to exercise with promptitude and judgment the existing powers in regard to the supervision and control of the milk supply, and also endeavour to secure amongst those engaged in the trade a greater sense of the responsibilities which rest upon them in connection with the occurrence of cases of illness in their households.

The Laboratory of the Scottish Asylums

THE third annual Report by Dr Ford Robertson, the Pathologist of the Laboratory of the Scottish Asylums, with the Report of the Board of Management, has just been published, and we are glad to again take the opportunity of calling attention to the important work done in this laboratory. During the year 1899, nine medical men received a laboratory course of instruction. Six of these were assistant medical officers of asylums in Scotland and England. The Library of the Laboratory is being more and more taken advantage of. All the principal neurological journals are to be obtained

from it, and they are purposely kept unbound so that single numbers may be sent by post to those desiring a loan of them. Only seven Reports were issued during the year on material sent to the pathologist, but Dr Ford Robertson explains that this was due to the fact that he was much occupied with the Text-Book of Pathology in Relation to Mental Diseases which he had undertaken to produce with the consent of the committee.

Dr Robertson states that it is proposed that the pathology of the acute insanities is to be made the next subject for systematic investigation in the Laboratory. This investigation, if undertaken, will extend over a number of years, as the researches in connection with it would necessarily be extensive.

As the object the promoters had in starting this laboratory cannot be too widely known and fully understood, we quote at length from Dr Robertson's Report. If any of our readers have influence with the millionaires who would be glad of advice as to how to dispose of some of their capital in such a way as to be a lasting benefit to mankind, this laboratory and similar schemes might be pressed upon their notice, for they would be enormously strengthened by liberal endowments.

The Report says :—

One of the non-medical members of the Board has specially asked me to insert in this Report some statement that may make it quite clear to all those in the same position as himself what are the objects of such a Laboratory as this, and what means are being employed in order to further them. Briefly stated, the great object that the promoters of this Laboratory have in view is to advance our knowledge of the essential nature of those disorders of the human frame collectively termed mental diseases, and the means that have up till now been employed for the furtherance of this object consist chiefly in the investigation of the structural changes affecting the nervous system in different forms of insanity, and in endeavours to encourage and aid the prosecution of such researches by the assistant medical officers of the associated asylums. But so brief a statement can scarcely serve to make it clear why this work is regarded by many as of such high importance as to require a central Laboratory in which to carry it on.

I shall therefore endeavour to explain the matter more fully. By most people at the present day it is not sufficiently understood that the great power, unfortunately restricted though it is in many directions, that the physician and surgeon now possess in combating disease is almost entirely the result

of the application of knowledge that has been acquired by scientific methods of observation. For years or centuries past, men have been patiently investigating such subjects as the chemical and physical properties of matter, the structure of the animal body, the nature of the normal vital processes, the causes of, and the structural and functional changes associated with, various forms of disease, the phenomena that these present in the living subject, and the action of various substances upon the animal organism in healthy and morbid states. These investigations—the labours of the chemist and the physicist, of the anatomist, the physiologist, the pathologist, the clinician and the pharmacologist, as well as of numerous other scientific observers—have led to the accumulation of a great mass of knowledge, the application of which in the prevention and relief of disease constitutes the medical and surgical art of to-day. Further advances in medicine and surgery depend essentially upon the progress of the sciences upon which they are based. If this is true for medicine in general, it is equally so for that special department of medicine which concerns itself with mental diseases. The sciences upon the progress of which the advancement of our knowledge of mental diseases and of the means of preventing and treating them chiefly depends, are especially physiology, psychology, pathology, pharmacology and clinical observation. Among these pathology occupies a place of special and indeed essential importance, and this for at least three reasons:—(1) Pathology concerns itself with the elucidation of the causes and processes of disease, and these must be understood regarding any particular morbid condition before it can be treated, or means taken for its prevention, by rational as distinguished from empirical methods. (2) Recent discoveries in the domain of the pathology and physiology of the nervous system have opened up for further research a vast and highly promising field which now only awaits the coming of the workers to survey it. (3) There are certain directions in which our knowledge of mental diseases and our power to treat them successfully can never make any important advances from the position in which they now stand without the aid of pathological research. In support of this last contention let me give two illustrations. When a case of acute mania is admitted to an asylum, very much can undoubtedly now be done for the benefit of the patient, but the measures that it is possible to adopt are chiefly of a palliative character. The disease has to run its course; our present knowledge of the forces at work in producing it is not such as to enable us to understand whether or not anything can be done to cut it short. But recent patho-

logical investigations have conclusively proved that at least certain cases of acute mania are essentially the result of the action of toxines, or poisons, upon brain-cells of comparatively weak resistive power. These toxines are known to be generated within the body; but as to their origin, nature, and varieties, we as yet know very little. It must be clear that until these points have been definitely ascertained we cannot know how to prevent the development of such toxines, or how to eliminate them from the body when formed. To investigate such matters is the province of pathology. As a second illustration, I would point to the common form of epilepsy. Here again recent pathological investigations have strongly tended to show that, after all allowance is made for the influence of congenital or acquired defects of brain, the disease has essentially a toxic basis. This difficult subject is being diligently investigated by pathologists now, more especially on the Continent, and many important particulars regarding it have been ascertained. It may be safely affirmed that it will be chiefly as the result of the knowledge such investigations will gradually furnish, that it will one day become possible to prevent or cure epilepsy. These are merely illustrations, but they might be multiplied almost indefinitely.

For practically all forms of insanity, it is true that pathological investigation has a large and essential part to fulfil in the advancement of the knowledge upon which alone successful treatment can be based. Unfortunately, such researches are exceedingly laborious; they present great, though not insurmountable, difficulties; they are inevitably slow in their progress; and they require for their successful prosecution not only costly apparatus and materials, but also a special training which the ordinary medical curriculum does not and cannot be expected to provide. It is evident, therefore, that they cannot advantageously be left entirely to the medical officers of asylums, whose medical and administrative duties naturally occupy most of their time. Certain individuals have to be set aside to give their whole energies to the work if it is to be carried on successfully. The rôle of the specialist in this department of pathological science is to add to our knowledge of the causes and essential nature of different forms of mental disease, and to keep himself informed of what other observers are discovering in the same field in order that he may know exactly how much has been surveyed, and how much has come so far within sight as to be waiting to be explored. Further, he ought from time to time to present in an intelligible form the results of these investigations, that the physician whose special work it is to endeavour to prevent and to cure insanity may be able to utilise

the knowledge gained thereby. In some even small measure to aid the attainment of these great ends is the purpose of the existence of this Laboratory.

So far I have endeavoured to give the information that has been asked of me. But I am certain if I were to leave the subject here I should convey to many a very false impression as to the position at present occupied by this Laboratory. I may therefore, perhaps, be permitted to pursue it a little further.

It is impossible to separate from these considerations regarding our aims and the methods by which we are at present endeavouring to carry them out, the question whether or not the efforts we are putting forth are adequate for the attainment of the great objects in view. There are at least two trustworthy standards by which we may judge of this matter—namely (1) the amount of work that is waiting to be done, and (2) what is being done by people in other countries. Regarding the amount of work waiting to be done, it may truly be said that it is almost limitless. As I have already mentioned, recent discoveries have opened up a vast and highly promising field for research in neurological science. A specially important recent development has been in the direction of chemical investigations bearing upon nervous diseases, and there are the strongest reasons for believing that results of the highest importance for the treatment of insanity will in future be obtained from investigations of this kind. These chemical researches cannot at present be undertaken in this Laboratory, because there is neither room nor equipment for their prosecution. But the present trend of progress in the pathology of insanity is such as to imperatively demand that they should be taken up. With regard to the second standard by which we may test this matter, it can be shown that Scotland, even when its comparatively small population is taken fully into account, stands a long way behind at least five other countries—viz., England, the United States, Italy, Germany, and France—in respect of the part it is taking in this work. In England the investigation of the pathology of insanity has been taken up in thorough earnest by the London County Council, which has built and equipped at great expense a splendid laboratory at one of its asylums (Claybury), and is now providing for the carrying on of the work at an annual cost of considerably over £1000. Moreover, the large asylums of Lancashire and Yorkshire have each a pathologist who is almost exclusively engaged in making researches in this field. In the United States the work is being diligently carried on in numerous asylums by patho-

logists on their staff. But the greatest effort at present being made in that country for the advancement of our knowledge of the pathology of insanity is represented by the Pathological Institute of the New York State Hospitals. In 1895 the Legislature authorised the establishment by the State Commission in Lunacy of "a department for the special study of mental pathology and allied subjects in connection with the State Hospitals, and primarily for their benefit." A comprehensive scheme was framed and duly carried into effect. The Institute, which is now in full working order, contains no less than eight departments—viz., those of Normal Histology, Pathological Histology, Cellular Biology, Bacteriology, Physiological Chemistry, Psychology, Anthropology and Comparative Neurology—each of which is in charge of a specialist, while the whole establishment is under the supervision of a Director. Over £7000 are yearly spent upon this Laboratory. In Italy, which has always been in the van of progress in neurological science, there are to-day scores of able observers whose enthusiasm for the study of everything connected with the nervous system seems to amount almost to a passion, and the pathology of mental diseases receives a full share of their attention. Ample facilities for the prosecution of such researches are given them; practically every Italian university and asylum has its institute or laboratory in which the work can be pursued. Likewise in Germany and in France researches upon the pathology of insanity are being made in numerous laboratories, from which most valuable contributions to our knowledge are continually being sent out. It is almost impossible to obtain accurate statistics regarding the cost at which this work is being carried on in these different countries, but the important question is rather—What facilities are being given for such investigations? Even when we look at the matter merely from this point of view, Scotland stands far behind these other countries that have been named. Seventeen out of its twenty-four asylums unite in the support of a central laboratory with a single worker, and one of these asylums has in addition a pathologist of its own. These being the facts of the case, it must be obvious that in Scotland we are not doing our just share in the great work of advancing the pathology of insanity, and that large quantities of most valuable material are being allowed to go unutilised.

It will naturally be asked—What should be done in order to remedy this state of matters? In giving my own views upon this question, I would emphasise the fact that researches in the pathology of insanity cannot now be limited to those pursued by histological methods. Chemical investigation has become

of equal importance, and must therefore also be employed. But the respective fields of neuro-histology and chemical physiology (upon which chemical pathology is based) are now so extremely wide that it is impossible for anyone to be a specialist in both. To ask one man to make himself thoroughly conversant with the present position of knowledge in these two departments of science, to keep himself informed of the advances that year by year are being made in them by other observers, to know thoroughly the complicated methods of research required to be employed in each, and to carry out systematic investigations so different in character, is to give him an impracticable task. Therefore, if this chemical work is to be taken up in our Laboratory, it should be assigned to a thoroughly trained chemical physiologist. It is scarcely necessary that in the matter of a central laboratory for the asylums of Scotland we should endeavour to emulate the Pathological Institute of the New York State Hospitals with its eight separate departments. But if we are to do our full share of this important work, we must make a larger effort than we are putting forth at present. We require at least two separate departments, a histological or anatomical, and a chemical. Each should be in charge of a specialist whose duty it is to make original investigations in his own field, and to encourage the prosecution of similar work by others, both in the Laboratory and in the various asylums. Further, some arrangement should be made by which men who are anxious to continue for a considerable time at research-work in the laboratory may be enabled to do so. At the Claybury Laboratory a similar need has to a certain extent been met through some of the Research Scholars of the Technical Education Board arranging to work there.

To carry out a scheme of this kind would cost at least double the amount that is being expended now. The provision of so large a sum for scientific work is undoubtedly a difficult matter for our Asylum Boards. But the amount would not be out of proportion to that which the London County Council and the New York State Commission in Lunacy are devoting to the same object now. There is only one other way in which, it seems to me, a solution of this difficulty may some day be found—namely, in the provision of an endowment. The work has indeed claims to which the attention of wealthy philanthropists may justly be directed, for its aim is to lessen the amount of one of the commonest and most terrible forms of human suffering. It were easy to believe that these claims only needed to be clearly represented to ensure a ready response in some quarter or other; for already in other respects in Scotland the cry of the mentally afflicted has not gone up in vain. In no country in the

world are the insane more humanely treated, their material necessities and comforts more amply and ungrudgingly provided for, their individual rights more jealously guarded by the law, or greater care taken to secure on their behalf the highest special medical skill that can be obtained.

Report of the Medical Officer of Health for the County of Fife

THE report for 1899 of Dr Nasmyth, medical officer of health for the county of Fife, shows that much progress was made during the year in improving the sanitary condition of the county. The number of infectious diseases was much lower than in the previous year, while a great deal has been done towards providing sufficient hospital accommodation. Tubercular diseases, we are told, account for one-seventh of the total deaths in the county, and Dr Nasmyth emphasises strongly the necessity for improvement in the hygiene of domestic animals and more especially in byres, as a means of diminishing the extent of this scourge.

Leaflets have been circulated over the county with instructions regarding the care of consumptives and the prevention of consumption. The detailed reports of the four districts of St Andrews, Cupar, Kirkcaldy, and Dunfermline, show that much is being done throughout the county to improve water-supplies and the condition of bake-houses, dairies, slaughter-houses, and dwelling-houses.

An epidemic of typhoid fever occurred in Burntisland in the beginning of September which was of interest from its mode of origin. The water-supply used by the persons attached was not the same, at least three different waters were used. The milk supply was, however, the same for all the cases save one. The dairy from which the milk was obtained was a very good one, well constructed and well ventilated, and no case of notifiable illness had lately occurred there. The water which was used on the farm came from a spring in the hillside, and was conveyed to the farm in a close iron pipe. The water was considered to be of very good quality. On examining the hillside, however, in the region of the spring, Dr Nasmyth found a large amount of fæcal pollution. He found that this had been deposited by quarrymen working in a quarry on the south side of the hill. The soil was open and porous, so that it was evident that pollution of the water was the natural result. No history of illness among the quarrymen could be obtained, but they were a constantly changing body of men. The water on analysis showed

free ammonia in the proportion of ·0048 grains per gallon, and albuminoid ammonia in the proportion of ·0020 grains per gallon. There were no nitrates or nitrites present.

Dr Nasmyth satisfied himself that the bacillus coli was present.

Meetings of Societies

Glasgow Medico-Chirurgical Society.

AT a meeting on April 6th, Dr Robert Kennedy showed the following:—

(*a*) A man who was operated on for cicatricial compression of the brachial plexus, causing total paralysis of the arm, and who had almost completely regained function. There had been a fracture at the surgical neck of the humerus, resulting in entire loss of movement of the arm, except a swinging movement by the pectorals and the latissimus dorsi. Sensation for touch and pain was lost in the lower forearm and hand. It was clear that the nerves were involved in the callus of the fracture. An incision was made, and the blood-vessels and nerves were found all bound down to the callus. The various structures were seriatim freed by careful dissection. Pulsation, which had been absent in the brachial artery, returned after the axillary artery was liberated. The day after the operation, some sensation to touch had returned in the forearm and hand. The first sign of returning power was observed in the thirteenth week after the operation. By the eighth month he could raise his arm above the level of the shoulder. At the time of his being shown to the Society, the fourteenth month, sensation was restored, also good voluntary power, except in the small muscles of the hand. The operation was done ten weeks after the accident.

(*b*) Photographs of a second case of brachial plexus paralysis, in which the compression was due to a large axillary aneurism, and in which Antyllus' operation was followed by complete restoration of sensation, and partial restoration of motion. This patient was a boy, aged 14 years. The tumour was the size of a large cocoa-nut, and typically an aneurism. The general condition of the boy was bad, and he suffered intense pain. At the operation the pectoralis major was lifted up, the pectoralis minor cut, and the artery above the aneurism exposed and tied. The sac was then opened and cleared out. Free hæmorrhage occurred, ceasing at once on ligature of the distal end of the artery. A rapid recovery was made. Sensation improved the day after the operation, and progressed to complete restoration

in eleven weeks. Voluntary movement was beginning to return, but the boy died from pulmonary tuberculosis three months after the operation. Antyllus' operation had been chosen, because it afforded a means of at once removing from the brachial plexus the pressure which was destroying it.

(*c*) A boy whose ulnar nerve was sutured three months after division, who recovered sensation the day following the operation, and who had now completely regained the use of his hand. He had been severely cut on the football field by a piece of glass, several tendons and the nerve being severed. The proximal end of one of the tendons had been sutured to the distal end of the nerve by the medical man who had first attended to the wound. When Dr Kennedy saw the boy there was characteristic ulnar paralysis. Fifteen weeks after the operation atrophy was much lessened and voluntary movements were returning. Ten and a half months after the operation he had full use of his hand.

(*d*) Two patients whose median nerves were sutured, one three months, and the other one month, after division, and who had now recovered the use of their hands. In both these cases a small neuroma was present at the site of suture.

In the discussion on these cases Dr Alexander Robertson asked if, at the operations, Dr Kennedy had observed much atrophy of the distal ends of the divided nerves.

Mr Clark said that long ago he had written a paper on the Primary Suture of Nerves. This was of course an axiom in practice—to suture nerves at once after their division. The results of secondary suture were inferior to those of primary. Long ago the results of secondary suture were inferior to what was obtained now. Dr Kennedy had done much work on the subject, and had demonstrated the fact, now well known, that sensation returned extremely quickly after rupture, indeed, that conduction of nerve force took place before there was time for any proper regeneration of nerve fibres to have occurred.

In case (*a*) it was important to note, as bearing on the prognosis in such cases, that a great degree of atrophy could be recovered from.

Dr Kennedy, in reply, said the most striking thing about the cut nerve was that there was no atrophy at all in its distal part. It was the same size as the proximal end. In all his operations on nerves, he had never seen atrophy of the distal end. Return of sensation set in very early in nearly all cases, usually in from one to three days. His belief was that the distal end of a divided nerve could by itself regenerate nerve fibres. After suture, following a long period of time from the division of the nerve, sensation was at once established, be-

cause the fibres were already laid down, so to speak, and no degeneration was present.

2. Dr J. M'Gregor Robertson and Dr J. Soutar M'Kendrick demonstrated the methods of making blood films, and showed a series of microscopic preparations, illustrating more especially leucocythæmia. The various methods of preparing and staining were described, and those best suited for quick clinical work. The varieties of the red and white cells, in health and disease, were described and demonstrated, and observations made upon the uses of these methods in diagnosis and prognosis, and in noting the effects of treatment.

Dr Carstairs Douglas discussed the paper, and mentioned some of his own experience in the subject, endorsing all that had been said as to the interest and value of such observations, and referring to the help afforded in making a differential diagnosis between tubercular meningitis and typhoid fever, as in the former there was a marked leucocytosis, a state of matters not observed so far in the latter. By this means he had seen a case of tubercular meningitis diagnosed while actually under treatment in a typhoid ward.

The methods were of value, too, in the prognosis of pneumonia, a low leucocyte count being of bad prognostic significance. He mentioned also the fact, amongst others, that diabetic blood stains differently from normal blood. Thus it will not stain with congo-red or methyl blue, while normal blood will. A staining test for the blood had been evolved in this way, of service in these cases of diabetes when sugar no longer exists in the urine.

3. Dr T. K. Monro and Dr Charles Workman contributed a note on a "Case of hæmorrhagic infarction of the intestine occurring in the course of enteric fever," and showed specimens.

The details of the case were given, which had been diagnosed after some time as enteric fever, on the evidence of continual fever, a few rose spots, loose yellow motions, and epistaxis. Shortly before death very severe abdominal pains set in, and perforation of the bowel was thought of, though the symptoms were hardly pronounced enough for that. The temperature then fell to normal, also the pulse and respiration, and there was slight general improvement, so that the patient was able to pass water and defæcate voluntarily, a great advance on the previous state of matters. In forty-eight hours the patient became worse again, passed blood from the bowel, and died from exhaustion. On *post-mortem* examination no peritonitis was found. There were numerous ulcers in the ilio-cæcal region, but none very deep. The commencement of the ascending colon was almost black, with a reddish tint, engorged with

blood, and displaced downwards. Dr Workman thought it was infarction from embolism of the mesenteric artery. There was found, however, thrombosis in the mesenteric veins. This, no doubt, explained the abdominal pains. The difficulty in diagnosis was interesting. Peritonitis was simulated. The condition was one of great rarity. It was not alluded to by Murchison, Keen, or Hare in their special works on the subject. Possibly it had not been recorded by any one at all.

Mr Clark had been called to the case with a view to operation; but, after seeing the patient and considering that there was no collapse such as would have occurred from perforation, and neither definite localised pain and tenderness nor diffuse pain and tenderness, he was unable to think it was a case of perforation, or one for operation at all, in the generally low condition the patient was in. If the operation for perforating enteric ulcer were undertaken indiscriminately, and in an unguarded manner, there would be a danger of operating when no perforation had occurred at all.

Glasgow Obstetrical and Gynæcological Society

AT a meeting on Wednesday, 28th March, Dr E. Arthur Gibson read notes of—

(*a*) A "case in which menstruation took place through a sinus in the groin," and showed a specimen. To relieve the condition he had opened the abdomen above, and parallel to, Poupart's ligament. The sinus passed down to the left side of the fundus uteri. The omentum and sigmoid flexure were adherent. There was no evidence of an ovary on the left side. The Fallopian tube formed the sinus, and this was removed, ultimate recovery ensuing. His first impression of the case had been that it was one of tuberculosis of the Fallopian tube, but no evidence of this was forthcoming. There was the fact in the history of the patient that some years before a "tubercular" gland had been removed from the groin, and he believed that that had really been a hernia of the ovary, and not a tubercular gland at all.

(*b*) "A case in which menstruation persisted after the removal of both ovaries." He had removed both ovaries from a woman for long-standing pelvic trouble, the left ovary containing pus. The menses had occurred normally thereafter. The sexual appetite was in no way impaired. The ovaries were shown to the Society. Pozzi had stated, Dr Gibson remarked, that such menstruation ceased in a few months. In this case it had persisted for five years. He had no doubt in his own mind

that he had removed both ovaries entirely, and a considerable part of the tubes also. He believed that this supported the theory that the tubo-uterine plexus of the sympathetic nervous system controlled the function of menstruation.

Dr Jardine, in discussing the cases, considered that in case (*a*) a hernial ovary had been removed long ago, as stated in mistake for a "tubercular" gland. He had seen menstrual flow occurring through stitch sinuses in cases of Cæsarean section, when the uterine stitches worked their way out through the abdominal cicatrix.

With regard to case (*b*), was it possible that there had been a third ovary. It was stated that this occurred, but he had never seen it.

Dr Edgar agreed with the explanation given of case (*a*). As for case (*b*), the most common cause of continuance of menstruation after ovariotomy was that a piece of ovary was left behind. He had left a piece of ovary on purpose sometimes, and menstruation continued. If the ligature was tied very close to the ovary a portion of the gland was very apt to remain in the abdomen. Still it was known, and in his own experience, that, in some cases where it was quite certain that both ovaries were completely removed, menstruation had continued. He was in agreement with the theory that menstruation was under the control of the sympathetic nervous system.

Dr Marshall agreed with the explanation of case (*a*). He had only once met with a case of ovarian hernia. With regard to case (*b*) he had seen a case of apparent menstruation after total hysterectomy. Such cases were recorded. The question then was, Where did the blood come from? Did it come from the vagina? It might do so, for that would not be more remarkable than the cases well-known of vicarious menstruation elsewhere.

Dr Russell had seen a case in which menstruation took place slightly and regularly through a small abdominal wound to which the ovarian cicatrix had become adherent. As for case (*b*), he had often seen operators in trouble in removing the ovary, and over and over again had noticed that a small portion of the ovary was left in the abdomen. This fact he considered to be the usual cause of persistence of menstruation after ovariotomy.

2. Dr Alex. M'Lennan showed "An instrument designed to prevent rupture of the perineum during parturition," and explained its mode of use and the cases in which it was suitable.

3. Dr Jardine read "Notes of eight labours complicated by tumours."

(1) A case in which a parovarian cyst had ruptured during forceps application without bad result.

(2) A woman eight months pregnant, when the question arose as to whether symptoms were due to the twisting of the pedicle of an ovarian tumour.

(3) A case of suppurating dermoid of the left ovary and obstruction of the bowels from adhesion. She had been in labour four days. The abdomen was much swollen. He had dilated the cervix manually and delivered the child by version, but it died. The mother was stimulated, and two hours after the delivery he had opened the abdomen. The bowel was found to be obstructed by adhesions and very dark in colour. The wall of the cyst gave way, and fœtid pus escaped into the abdomen. The patient did not recover. Early in the pregnancy the woman had been urged to have her tumour removed, but she would not permit it.

(4) Several fibroid tumours. The os was fully dilated and the head presenting when he saw the case, and he delivered by forceps. The chorion was adherent all over and had to be removed bit by bit.

(5) Fibroid tumours of the fundus uteri. The case clinically suggested a twin pregnancy. The placenta could not be expressed. It was found that another fibroid tumour—a submucous one—lower down, blocked the way. The chorion was adherent all over, and had to be removed piecemeal by the hand.

(6) A case in which there was ante-partum hæmorrhage, the head partially engaged, the cord prolapsed and pulseless. A tumour low down blocked the way. Perforation was performed and delivery by forceps, a good recovery of the patient ensuing. Afterwards there was found in the pouch of Douglas a tumour, either a fibroid or a dermoid of the ovary.

(7) A case in which there was a subserous fibroid about the umbilicus, and a large hard tumour filling the pelvic cavity and completely blocking the way. The woman was collapsed. A Porro's operation was performed. The tumour was so jammed in the pelvis that much trouble was experienced. Death occurred in forty hours from delayed shock.

(8) Numerous cauliflower masses in the vagina, gonorrhœal in origin, and very septic. The woman was in labour. He removed all the tumours by excision, sewing up the wounds as he progressed, and delivered the child by version. The patient made an uninterrupted recovery.

Dr Edgar, in the discussion which followed, referred to several cases of fibroids complicating pregnancy and labour in his experience. In one case of two large fibroids complicating

pregnancy, he had done a hysterectomy at the fourth month with complete success.

The dangers in such cases were hæmorrhage, obstruction to the labour, and septicæmia. Portions of placenta might remain adherent to fibroid tumours and cause septicæmia.

With regard to ovarian tumours, he inclined to remove them during pregnancy for fear of torsion of the pedicle and peritonitis.

Dr Richmond related a personal experience of a large fibroid which had simulated twins.

Dr Munro Kerr laid stress on the injuries which tumours complicating labour might receive and the degeneration and necrotic changes which might occur in them consequently, and the dangers arising therefrom.

Forfarshire Medical Association

AN ordinary meeting of the above Association was held on April 6th in the University College, Dundee. Dr C. Templeman, Vice-President, in the chair.

Professor MacEwan showed a private of the Royal Highlanders who was suffering from the effects of a Mauser bullet wound received at Magersfontein. The wound of entrance was to the right of the seventh cervical vertebra, and wound of exit was between the tenth and eleventh ribs on the right side. There was some wasting of the scapular muscles, right trapezius, supra and infra-spinatus muscles, some anæsthesia over right shoulder, and also some difficulty in deep breathing, with slight cough.

Professor Kynoch showed several specimens: (1) two multilocular ovarian cysts removed by abdominal section; (2) papillomatous ovarian cyst, the size of a child's head, causing retention of urine and removed by section through the post-vaginal fornix; (3) a uterus with a calcified fibroid in the anterior wall simulating vesical calculus and removed by vaginal hysterectomy; (4) adeno-carcinoma of the body of the uterus removed by vaginal hysterectomy.

Dr J. S. Y. Rogers gave the notes of a case of labour complicated by a dermoid cyst. He dealt with the difficulty experienced in this case and the prognosis.

Dr R. C. Buist gave further notes of the case and the treatment adopted for the removal of the cyst after delivery. He showed the cyst and made some remarks on the specimen.

Dr Steele Moon and Professor Kynoch spoke on the case.

Dr Rogers briefly replied.

Dr Templeman gave a short summary of the seventy cases

of typhus fever he had had under his care in the Fever Hospital during the last two years.

Dealing first with the ages, twenty-one were between 5 and 10. There were none between 15 and 20, only one above 60.

Then he spoke on the mortality. Of forty-two under 30, only one ended fatally; of twenty-three over 30, eleven were fatal.

The time of the year when most cases occurred was next referred to. The incubation period in a large number was longer than twelve days.

The clinical features were next dealt with, and the complications which occurred.

Dr Steele Moon, Professor Kynoch, and Dr Husband of Rajpootana, spoke on the paper.

Dr Templeman replied.

Obituary

Sir Douglas Maclagan, M.D., LL.D.Edin., LL.D.Glasg., F.R.C.P.Ed., F.R.C.S.Ed., F.R.S.Edin.

By the death of Sir Douglas Maclagan a notable link between the present and the past of Scottish medical life has been removed. He was a representative of the type of learned physician, and of the cultured, upright and high toned gentleman, which modern specialism and the keenness of a widespread commercial spirit do not tend to develop or to encourage in these later days.

Andrew Douglas Maclagan was born in the year 1812, in Ayr, and was the son of a medical man. His father, Dr David Maclagan, served in the Peninsular war, and held the position of Surgeon-in-ordinary to the Queen in Scotland and Physician to the Forces. He also had the remarkable distinction of being President both of the Royal College of Physicians of Edinburgh and of the Royal College of Surgeons of Edinburgh. Andrew Douglas Maclagan was the eldest of seven sons, another of the sons being at present the Archbishop of York.

At the age of nineteen he became a licentiate of the Royal College of Surgeons of Edinburgh, and soon after graduated in medicine at the University. He then studied in Berlin, London, and Paris for several years, and on returning to Edinburgh was elected assistant surgeon to the Royal In-

firmary. He soon however abandoned surgery, and in 1845 he became lecturer on *materia medica* in the Edinburgh extra-academical school. In 1857 he was retained for the defence in the famous Madeleine Smith case. In 1862 he was appointed to the chair of Medical Jurisprudence and Public Health on the death of Professor Trail. His name is also connected with another famous criminal case—the case of Dr Prichard, who poisoned his wife and mother-in-law. Maclagan's report on the organs of the victims was one of the most important pieces of evidence against the accused. In fact, Professor Maclagan became regarded as one of the leading toxicologists in Great Britain, and his services were constantly made use of in Scotland in the elucidation of cases requiring chemical knowledge combined with common sense.

He shared with his father the honour which has been conferred on no other man, of being elected to the presidency of both the Royal College of Physicians of Edinburgh and of the Royal College of Surgeons of Edinburgh. He was President of the Royal College of Physicians in 1864 and of the Royal College of Surgeons in 1884. He also at one time held the position of President of the Royal Society of Edinburgh. In 1886 he received the honour of knighthood.

His work as a professor he did with all his might. The subject was congenial, and fitted in well with his knowledge of practical chemistry and botany, and with the experience he had already acquired as a practitioner as well as a medical jurist. His lectures were delivered in a terse and intelligible style highly valuable to a class composed both of legal and medical students, and the subject was handled with becoming tact and gravity. From an early period Dr Maclagan began to devote attention to public health as a part of his course, and delivered a series of lectures upon it to his class of medical jurisprudence at the end of each summer session. The subject however grew so large, that he was compelled to carry it over as a special short course during a portion of each winter session. In this way he laid the foundation of a new department which has come to occupy an important position in the University's scientific work, and which he lived to see grow into a separate Chair with his former assistant, Dr Hunter Stewart, as its first occupant.

During a large portion of his professoriate Dr Maclagan took part in clinical teaching in the Royal Infirmary. In this work he took a lively interest, and if his clinical methods were less elaborate than those of some of his colleagues, he excelled most of them in his therapeutic knowledge and resources. He always commanded a goodly following of students.

Not a little of the strength of Dr Maclagan's position as a professor lay in his personal character. His high character gained for him confidence and respect, while his geniality and his many gifts endeared him alike to colleagues and pupils. The same virtues characterised his private life and gave him troops of friends.

Sir Douglas Maclagan was a man of many talents. He had a considerable knowledge of music, and at one time published a book of verses, "Ungæ Canoræ Medicæ" among which is his "Alma Mater" which is known among Edinburgh students to music of Sir Herbert Oakeley's setting. He was a good classical scholar. He was an enthusiastic sportsman, he early joined the volunteer movement, and he was Surgeon-General of the Queen's Bodyguard for Scotland.

After a life unusually prolonged in its activities, he had for a few years past retired from all work, and he quietly waited for the end, which it is pleasing to know came peacefully to him. He was buried in the Dean Cemetery on the 9th April.

Sir William Priestley, M.D., LL.D., F.R.C.P.Edin., F.R.C.P.Lond., M.P.

By the death on April 12th of Sir William Overend Priestley, M.D., Member of Parliament for the Universities of Edinburgh and St Andrews, a notable leader in obstetric medicine has been removed.

William Overend Priestley was born in 1829 at Churwell, near Leeds, and was educated at the University of Edinburgh and King's College, London. His college career was brilliant, and in 1853 he took the degree of M.D. at the University of Edinburgh, being Senate Gold Medallist of his year. He was for some time thereafter assistant to Sir James Simpson.

In 1856 he went to London, where he began practice as

a physician, being appointed lecturer on Midwifery at the Middlesex Hospital, and at the Grosvenor Place School of Medicine. He married in the same year a daughter of Robert Chambers of Edinburgh.

In 1863 he was elected Professor of Midwifery and Diseases of Women and Children in King's College, London, and on the resignation of Dr Arthur Farre he became Physician-Accoucheur to that hospital. In 1872, on resigning his post at King's College, he was elected a Consulting Physician to the hospital, and later was made an Honorary Fellow of the College. He was selected by the Queen as Physician-Accoucheur to the late Princess Alice of Hesse, and to Princess Christian of Schleswig-Holstein, and in 1893 he received the honour of knighthood from Her Majesty. In 1896 he was elected without contest as Parliamentary representative for the Universities of Edinburgh and St Andrews in room of Sir Charles Pearson, who was elevated to the Scottish Bench.

Sir William Priestley was elected a Fellow of the Royal College of Physicians of Edinburgh in 1858, and a Fellow of the Royal College of Physicians of London in 1864. He was an honorary member of the Obstetrical and Gynæcological Societies of Berlin and Leipzig, of the Obstetrical Society of Edinburgh, and of the American Gynæcological Society, and a Vice-President of the Paris Medical Society. He edited Sir James Simpson's collected works along with Storer. Priestley's original work was sound, and his success in practical work marked. Sir William Priestley was a man of genial and kindly disposition, with a keen sense of humour and a ready sympathy, which won for him many friends.

Editorial Notes and News

The Spring Graduation at Edinburgh University

At the Spring Graduation ceremony of Edinburgh University which took place on April 14th in the M'Ewan Hall, the honorary degree of doctor of laws was conferred on two members of the medical profession—Mr C. D. F. Philips, M.D., Aberdeen, LL.D., for many years lecturer on therapeutics in the Westminster Hospital, London, and Mr Anderson Stuart, M.D.,

Edinburgh, professor of physiology in the University of Sydney. This degree was also conferred among others on Miss Eleanor A. Ormerod, F.R.Met.Soc., F.E.S., and on his excellency, the Hon. Joseph Hodges Choate, Ambassador for the United States of America at St James'. The address to the graduates was delivered by Professor Seth of the chair of moral philosophy. The service in St Giles' Cathedral was conducted by the Rev. Professor Patrick, the Rev. J. B. Hastings, and the Rev. A. R. Yeoman.

University of Edinburgh—Faculty of Medicine

The following is the official list of candidates who have passed the examinations undermentioned for degrees in Medicine and Surgery:—

First Professional Examination.—Farhat Ali, Mark Ashkenazie, Agnes Balfour, C. M. Begg, A. R. Berrie, J. M. Beyers, A. S. L. Biggart, F. H. Borthwick, F. S. Brennan, James Brennan, Angus Buchanan, R. B. Calwell, M. C. Cowper, R. W. Craig, C. S. Crichton, George Cunningham, W. H. Davison, J. R. Dobbin, T. H. Easton, C. G. Edmonston, J. J. H. Ferguson, F. S. B. Fletcher, N. C. Forsyth, R. H. Fothergill, H. M. Gillespie, J. A. Glover, L. C. V. Hardwicke, George Harrison, Ephraim Henderson, M.A.; G. S. Husband, R. J. Irving, S. A. Johnston, I. C. Keir, John Kirk, E. A. Klein, W. E. Knight, C. A. Lawrence, C. D. Lochrane, J. B. Lockerbie, A. P. G. Lorimer, John M'Donald, R. J. Mackessack, M.A.; Elspeth M. Macmillan, D. P. Marais, J. B. Mears, C. R. P. Mitchell, Helen Morison, T. B. Mouat, C. F. A. Oberländer, Archibald Oliver, Frank O'Neill, E. H. Price, Labbhu Ram, William Readman, A. E. C. Rees, D. G. Reid, A. O. P. Reynolds, W. H. Robertson, L. C. Robinson, John Saffley, S. W. Smith, W. A. Wilson Smith, H. A. Stewart, H. J. Stewart, A. C. Strain, Annie F. Theobalds, T. T. Thomson, E. A. Turpin, D. C. Welsh, Leonard West, R. M. Mainwaring White, D. P. D. Wilkie.

Second Professional Examination.—A. K. Baxter, Henry Beveridge, B.A.; Breadalbane Blacklock, C. H. Bösenberg, J. R. Bosman, E. A. Braine, C. B. Brand, R. D. Brown, Archibald Brownlie, D. M. Callender, M.A.; C. M. Campbell, M.A.; D. G. Carmichael, T. F. Cavanagh, A. M. Caverhill, J. H. Connolly, P. D. Cremona, T. M. Cuthbert, F. I. Dawson, Jan Dommisse, H. J. Dunbar, J. F. Duncan, John Dunlop, S. A. Ellerbek (with distinction), S. C. Ellison, A. N. Fell, A. P. R. Fennell, Sidney Gilford, Archibald Gillespie, A. M. Gloag, D. P. Goil (with distinction), Wilbert Goodchild, Andrew Grant, J. P. Grant, J. A. Gray, J. F. Haegert, T. B. Hamilton (with distinction), Howard

Harding, A. W. M. Harvey, Henry Herd, M.A.; Matthew Holmes, C. H. Houghton, R. C. Irvine, J. M. Johnstone, Harold Kerr, James Kirkwood, H. A. Knight, W. S. Laidlaw, Norah Lenwood, William Lillico, J. C. London, R. C. Low, J. G. M'Caughey, D. R. MacGregor, B.Sc.; I. N. M'Keand, V. C. M'Laren, Evan Macmillan, R. P. M'Neil, C. E. Marshall, W. E. Marshall, J. B. Mason, J. K. Matheson, Paul Mathews (with distinction), R. J. Mayberry, Thomas Mill, W. S. Milne, Anthony Moll, J. H. Montgomery, S. A. Moore, Robert Morison, Robert Murray, Charles Nelson, William Newlands, E. G. O. Nixon, T. H. Osler, John Politachi, A. B. dos Remedios, H. H. Robarts, A. B. Ross, T. W. E. Ross, A. J. Rowan, A. L. Roxburgh, James Scobie, J. S. Sewell, A. M. Sharp, M.A.; E. S. Sharpe, C. J. Shaw, W. F. Smeall, J. M. Smith, M. S. Sodhi, D. L. Wall, B. P. Watson (with distinction), John Weir, S. A. K. Wilson.

Second Professional Examination (*Old Regulations*).—M. N. Bose, M. G. I. Cadell, Shaik Dawood (with distinction), J. H. Meikle, M.A., B.Sc.; A. C. Peterson, D. R. Rees, W. H. Swaffield.

Third Professional Examination.—May Agnew, J. F. Allan, C. M. Anderson, R. T. Baillie, A. C. Begg, R. A. Belilios, V. C. de Boinville, R. J. Bradley, A. J. Brock, James Brocket, E. E. Brown, G. M. Brown, William Brown, James Brownlee, Launcelot Bruce, R. W. Buchanan, Henry Buist, G. C. Burgess, E. P. Calder, Kirkland Chapel, C. C.Choyce, P. T. Copeland, W. J. Crow, B. M. Cunningham, John Dalgleish (with distinction), M. S. Dickson, W. E. C. Dickson, B.Sc.; T. G. B. Dodds, Henry Dodgson, Charles Douglas, J. L. Duncan, W. A. Duncan, J. M. Dupont, J. R. Edward, William Eggeling, J. W. Falconer, Stephen Garvin, E. M. Glanville, R. A. Glegg, Thomas Graham, R. M. Grant, A. L. Gurney, A. R. Hallam, James Halliday, A. M. Hamilton, William Hamilton, N. C. R. Hansen, R. A. J. Harper, E. R. Henderson, D. C. Henry, C. W. Howe, Andrew Hunter, M.A., B.Sc.; A. E. Hunter, A. R. Johnston, Edgar Jones, A. C. Keay, J. P. Kennedy, P. V. Langmore, A. R. Khan Lauddie, William Lee, J. G. M'Bride, J. B. M'Cutcheon, William MacDougall, P. F. M'Farlan, George M'Farland, F. D. S. MacKenzie, John M'Keszie, K. D. C. MacRae, William Mair, A. Malcolmson, A. F. G. Masson, E. C. C. Maunsell, G. R. Mill, William Miller, M. C. Morgan, John Morrison, David Munro, J. G. Munro, G. S. Murray, Peter Murray, D. A. Ogilvie, J. F. Orr, J. L. Palmer, W. S. Patton, S. G. Peill, George Pereira, H. G. Pesel, L. W. Pole, W. H. Prentice, M.A.; H. St J. Randell, H. M. Roberts, G. C. I. Robertson, M. W. Robertson, W. A. Robinson,

Laurence Rundell, C. W. Saleeby, T. B. Shaw, M. M. Smith, T. A. Smyth, Kate Southon, H. L. Shark, A. J. de Spiganovicz, M. M. Stevenson, D. A. Stewart, T. L. Thomson, E. A. Walker, A. R. Wallis, Edwin Wells, T. S. B. Williams, F. E. Wilson, James Woods, David Young, R. T. Young.

Royal College of Physicians of Edinburgh, Royal College of Surgeons of Edinburgh, and Faculty of Physicians and Surgeons of Glasgow.

The quarterly examinations of the above Board in Edinburgh were concluded on 10th April, with the following results:—

First Examination, Five Years' Course.—Of twenty-four candidates entered the following nine passed the examination:—William Hawkley Woodger, Newcastle-on-Tyne; Lilian Mary Grandin, Jersey; William Black Hendry, Edinburgh; Leo Ferdinandi Bianchi, Tunbridge Wells; Joseph Stark, Forth; Vivian St John Croley, India; John St Aubyn Maughan, Grimsby; William James Purves, Hartlepool; and Charles Ewbank Foot Gaitskell, St Leonards-on-Sea; and three passed in Physics, six in Elementary Biology and two in Chemistry.

Second Examination, Four Years' Course.—Of five candidates entered the following four passed the examination:—Samuel Graham Kinloch, Manchester; Patrick James Kerwin, Cork; James Levack, Caithness; and Robert Ellsworth Turner, Rock Ferry.

Second Examination, Five Years' Course.—Of twenty candidates entered the following fourteen passed the examination:—Cecil Richard Martin, Calcutta; John O'Regan, Mitchelstown; William Sam Rainey Dick, Ballynafil; Reginald Herbert Brierley, Rochdale (with distinction); John Dick, Edinburgh; Herbert Frederic Walker, Tasmania; Hamilton Watson, Shetland; William John Shorten, County Cork; William Vincent Sutton, Cork; William John Gray, Portobello; Henry Carlaw, Aldershot (with distinction); James Harvey Stewart, Alford (with distinction); Robert Walter Joseph Pearson, Ennis; and Hermann Sperling Christoffeloz, Ceylon.

Third Examination, Five Years' Course.—Of twenty-five candidates entered the following sixteen passed the examination:—Harry Findlater Wilkin, St Helens, Lancashire (with distinction); Alec Boswell Timms, Australia; Arthur Thomas Hoskins, Victoria; Gopal Chander Ghose, India; Herbert Ernest Jardine Batty, India; Thomas Huston, Carrickfergus; William Henry Eden Brand, London; Robert Thompson, County Derry; Helen Mary Serjeant, Huntingdonshire; Kenelym Cobham Edwards, Pelham; William Cowan Brown, Gateshead;

Benjamin Gowing, Salisbury; Alfred Lambie White, Dundee; John Walker Wood, Edinburgh; James Thomas O'Connor, Edinburgh; and George Kee, Greenock: and two passed in Materia Medica.

Final Examination.—Of fifty-nine candidates entered the following thirty passed the examination and were admitted L.R.C.P.E., L.R.C.S.E., and L.F.P. & S.G.:—Fanny Wood, Ireland; Joseph Henry Bennett, Bombay; William Daley, Runcorn; Robert Orford Jones, Llanfyllin; Geoffrey Charlstrom Hobbs, London; George Day, Norwich; Charles Henry Farquharson, Jamaica; William Pattullo, Alyth; Robert M'Laren, Edinburgh; Ralph Frankland Morris Fawcett, Whitehaven; Elizabeth Morton Johnston, Edinburgh; Jessie Jane Graham, Dunfermline; Andrew Graham, Peebles; Walter Percy Warburton, Leeds; William Thomas Finlayson, Edinburgh; Francis Wilfred Harlin, Brisbane; George Potts, Cheshire; Donald M'Donald Robertson, Perth, Ontario; Victor Adolphus Hart, Barrie, Ontario; Charles Glendinning Thomson, Hiawatha, Ontario; Thomas Malcolm Russell Leonard, Bangalore; William Arthur Benson, Co. Durham; Edith Anna Wynne-Edwards, Montgomeryshire; Henry Francis Stilwell, Hillingdon; John Robertson, Perthshire; James Henry Moag, County Down; Austen Andrew Bearne, Devonshire; Albert Ernest Salkeld, Victoria; Edward Bennett, Lancashire; and Vivian Sljohn Croley, India; and two passed in Medicine and Therapeutics, one in Surgery and Surgical Anatomy, two in Midwifery, and four in Medical Jurisprudence.

University of Aberdeen

The Winter Graduation Ceremony in connection with Aberdeen University took place on April 4th.

HONORARY DEGREES.—Among the recipients of the Honorary Degree of LL.D., was Dr J. Mitchell Bruce, London.

ORDINARY DEGREES.—The following is the list of Ordinary Degrees conferred in the Faculty of Medicine:—

I. *Degree of Doctor of Medicine* (*M.D.*).—Frank Beaton, M.B., Hirst, Morpeth. Thesis—"Entero-colitis of Infants: its ætiology and treatment." Hugh Gordon Cowie, M.A., M.B., Turriff. Thesis—"Clinical Notes on Diphtheria." * Arthur B. Dalgetty, M.B., Madabpore, India. Thesis—"Water-Itch." George B. Scott, M.B., Westbourne Terrace, London. Thesis—"Asthma in Children: its pathogeny and its varieties; two cases." David Simpson, M.A., M.B., Major I.M.S., Comibatore, India. Thesis—"Elephantiasis Arabum."

* The Thesis of Mr A. B. Dalgetty was considered worthy of "Commendation."

II. *Degrees of Bachelor of Medicine* (*M.B.*) *and Bachelor of Surgery* (*Ch.B.*) (*New Ordinances*).—* Henry Fraser, 82 Bon-Accord Street, Aberdeen (with first class honours); * Henry Peterkin, M.A., 51 Queen's Road, Aberdeen (with second class honours); Herbert Bell Tawse, 8 Argyll Place, Aberdeen (with second class honours); Arthur Westerman, 104 Leslie Place, Aberdeen (with second class honours); William John Ironside Bruce, The Castle, Dingwall; William James Caie, M.A., 61 Albury Road, Aberdeen; Robert Carnegie, 25 St Mary Street, Peterhead; John Catto, The Manse, Fintray, Aberdeenshire; Arthur Robertson Davidson, Kirkhill, Ellon, Aberdeenshire; Hugh Allan Davidson, Dunnydeer, Insch, Aberdeenshire; John Watt Duncan, M.A., 14 Esslemont Avenue, Aberdeen; Sylvester Davidson Fairweather, M.A., Mintlaw, Aberdeenshire; George Greig Farquhar, 34 Braemar Place, Aberdeen; John Forbes, Mains of Fyvie, Fyvie, Aberdeenshire; John M'Calman Fraser, M.A., 16 Belvidere Crescent, Aberdeen; Kenneth Fraser, 83 Church Street, Inverness; William Nusum Houghton, 21 Alexander Street, Bayswater, London, W.; Wilfrid Ernest Lawson, 26 Argyll Place, Aberdeen; David Mitchell Macdonald, 163 Crown Street, Aberdeen; John M'Kenzie, M.A., Dunmail, Cults, near Aberdeen; Ernest Hunt Nash, Holmleigh, Stafford; * William Douglas Pringle, Alexandria, Egypt; John Richardson, Laurel Villa, West March, Monifieth, Forfarshire; James Andrew Simpson, Alford, Aberdeenshire; Harold Traill Skae, 16 Dundas Crescent, Kirkwall, Orkney; * Robert Alexander Slessor, M.A., 62 Bon-Accord Street, Aberdeen; Charles Lawson Smith, 21 Belvidere Street, Aberdeen; Harry Emslie Smith, Summerhill, Stocket, Aberdeen; Arthur Leslie Sutherland, M.A., 3 Polmuir Road, Aberdeen; James Taylor, M.A., Toux, Mintlaw, Aberdeenshire; James Miller Swanson Wood, Laurel Villa, Victoria Road, Torry, Aberdeen.

III. *Degrees of Bachelor of Medicine* (*M.B.*) *and Master in Surgery* (*C.M.*) (*Old Ordinances*).—Joseph Hadfield, The Vicarage, Hadfield, near Manchester; Francis Walker Moir, Baillieswells House, Cults, near Aberdeen; Douglas Whimster Keiller Moody, 50 High Street, Montrose; William Mitchell Smith, Bank House, Strichen, Aberdeenshire; William Peters Young, 47 Argyll Place, Aberdeen.

IV. *Diploma in Public Health.*—James M'Rae Cowie, M.B., Turriff; † James Dawson, M.A., M.B., Rathen; Alfred James Hynd, M.B., Aberdeen; James Alexander Stephen, M.A., M.B., Elgin; Alexander Urquhart, M.A., M.B., Old Meldrum.

* Passed Final Examination "With Distinction."
† "With Credit."

The Parliamentary Representation of Edinburgh and St Andrews Universities

By the death of Sir William Priestley, a vacancy has occurred in the Parliamentary Representation of Edinburgh and St Andrews Universities. Sir John Batty Tuke, M.D., of Saughton Hall Asylum, Edinburgh, has been adopted as the conservative candidate, and he has issued his address to the electors. In the address he intimates that the government will receive his support in the prosecution of their policy in South Africa, that he is opposed to Disestablishment, and that he does not propose to relinquish practice, although he will make it his first duty to fulfil his parliamentary engagements. Sir John is likely to be returned unopposed, for so far there is no other candidate in the field.

Royal Colleges Golf Club.

The annual match between the Edinburgh colleges for the custody of the Loving Cup was played at Gullane on March 28th.

Neither college was at full strength, the war and university examinations engaging leading players in both teams. The day was pleasant, and the links in good order except for the water which filled many of the bunkers, and lay "casually" in too many places. When the scores were added up, it was found that the Surgeons had once more shown their superiority and regained possession of the Cup. It was but human nature that the Edinburgh Physicians should blame the Glasgow Professors for the loss.

Scores.

R. C. P.		R. C. S.	
A. D. R. Thomson	3	R. H. Blaikie	0
T. R. Ronaldson	0	W. A. Finlay	4
R. Stockman	0	J. M. Cotterill	7
R. Muir	0	G. Mackay	7
R. Thin	0	D. Menzies	1
P. A. Young	3	C. W. Cathcart	0
G. H. Melville Dunlop	0	E. Carmichael	1
A. Ballantyne	2	D. Waterston	0
D. Noël Paton	7	Russell Wood	0
G. L. Gulland	0	J. Shaw M'Laren	2
Norman Walker	6	F. M. Cadell	0
R. Lucas	0	J. W. B. Hodsdon	8
W. Fordyce	1	C. W. Macgillivray	0
G. A. Gibson	0	R. Kirk	3
T. S. Clouston	0	P. Hume Maclaren	2
	22		35

Majority for R. C. S., 13 holes.

New Books and New Editions

An American Text-book of Surgery. Edited by WILLIAM W. KEEN, M.D., LL.D., and J. WILLIAM WHITE, M.D., Ph.D. Third Edition. 2 volumes 8vo, pp. 1228. Illustrated. London: Rebman Publishing Company, Ltd. 1899. Price 42s.

A BOOK such as Keen and White's Text-book of Surgery, which has reached its third edition, does not require a detailed review. The plan and scope of the book are well known, and the articles are of the quality that one might expect from such eminent authorities as the editors have chosen as contributors. "Of the two former editions of the American Text-book of Surgery, there have been sold nearly 29,000 copies. This, and its adoption as a text-book in over one hundred medical colleges, have been gratifying evidence of the approval of the profession, and a stimulus to the authors to keep the work abreast of the times by another careful revision" (Preface to third edition). That this success is in many respects deserved no one will deny, but that the book would benefit from still further "stimulus to the authors" seems to us to be no less evident. It is the unevenness in quality that has always made this text-book an "irritating" one to refer to. The reader refers, say, to the chapters on *Surgical Bacteriology* and *Inflammation*, and is pleased with the clear, if condensed, account of the various organisms, and of the stages of that process, and is charmed with the coloured plates which illustrate these subjects; but when he skims over the article on Orthopedic Surgery in quest of the treatment of *Dupuytren's Contraction*, he alights on this too straitened strip of information: "Treatment.—Left to itself, the palmar contraction always gets worse, and it can be relieved only by operation. The skin should be divided by a linear wound, or reflected in a v-shaped flap, base downwards, and the contracted tissue dissected out; an operation which of late years has been frequently done with success. The hand and forearm are placed upon a palmar splint for a few days." This would scarcely suffice for a student preparing for an examination; as guidance to a practitioner it is almost useless. Nor is the advice sound; it would be interesting to watch the progress of a case in which the hand was placed "upon a palmar splint *for a few days*." It may be said that this is not a text-book of operative surgery. But, then, in that case, why are the details of the operation for cleft palate, for instance, described with such minute fulness? For some reason or other, *Hydrocele* always obtains ample discussion in surgical text-books; but

why in "Keen and White" should two pages—with a capital diagram of that intricate and difficult operation, "tapping a hydrocele" (!)—be devoted to the treatment of this condition while the treatment of *Tubercular Orchitis*, a very frequent and really serious affection, gets but nine lines? Why does *Hematocele* obtain two paragraphs, and *Spermatocele* none? Compare the section on *Diseases of the Spermatic Cord* with, say, that in *Tillmanns*, and the defects will be apparent.

On the other hand, the book contains many admirably written and beautifully illustrated articles. The article on the *Surgery of the Nerves* is excellent, clearly written, well illustrated, and with detail sufficient to guide even an adventurous practitioner; the pages dealing with *Neuralgia* are good, and under *Treatment* the reader is referred to a judicious paragraph in the article on the *Surgery of the Spine*, dealing with the question of intra-spinal section of the posterior roots. In the article on *Fractures*, the diagnosis between the two forms of fracture of the neck of the femur is considered in a sensible passage, and we are glad to find the following: "The diagnosis between the two varieties cannot always be made with certainty. . . . The opinion once very widely held, and even now occasionally encountered, that the differential diagnosis can be made by consideration of the age of the patient . . . or of the way in which the violence was received . . . has proved to be wholly untrustworthy." But previous editions have made readers familiar with these.

Turning to some of the articles which the editors tell us have been revised or added, under *Inflammation* we are glad to see "Leucocytosis" is not omitted; while some more space might have been devoted to it, what is said is clear, as, for example, "Virulent infections well resisted cause the greatest leucocytosis." "Where the infection is very mild, or where it is so overwhelmingly violent that the resistance of the system is easily overpowered, the leucocyte count may be normal or subnormal." "In a general way the course of the leucocytosis follows that of the temperature; but the leucocytosis may increase while the temperature falls, and in some cases (*e.g.* in appendicitis) the increasing leucocytosis may be evidence of a spreading peritonitis, which temperature and other signs would not indicate." Under *Serumtherapy* most will agree with this—"With this (streptococcus) serum some, though by no means constantly, favourable reports have been obtained in streptococcic septicæmia. The question is an open one, whether the good results did not arise from the simple stimulative action of the horse-serum upon the tissue-cells, especially as equally favourable results have come from the use of the serum in cases in which

there were probably no streptococci present." And with regard to immunisation from staphylococcus poisoning—"The difficulty in the way of the solution of this problem seems to lie in the absence of any immunity conferred by this organism upon the human subject." There are admirable articles on *Gastro-intestinal surgery*, *Diseases of joints*, and *Surgery of the genito-urinary tract*. For instance, in the first of these there is a capital article on appendicitis, with a judicious summary of the general question by White. In the last there is a full and, as it seems to us, a wise consideration of the subject of strictures of the urethra, with a clear and sensible division of the various types and the suitable treatment. But the following, though refreshingly vigorous, must not pass without some protest. "Extravagant claims have been made as to the usefulness of the instrument (the cystoscope), and are not likely to be fully realised, but it has taken an assured place in the armamentarium of genito-urinary surgeons. Its congener, the electric urethroscope, is of far less value, and has been much abused by novices and over-eager investigators, who have thus far added but little by its means to our knowledge of urethral conditions."

We may be allowed to add the following criticisms. Insufficient immobilisation is given as the main cause of *Non-union in fractures*; but should the intervention of muscle between the fragments of bone not obtain an equal or even a superior place? And should not Bier's "congestive" treatment find a place amongst the remedies? The description of *Hammer-Toe* is clumsy, though accompanied by a good diagram, and amputation should not be included amongst the methods of treating the affection. On page 709, under *Suturing of Wounds of the Stomach and Intestines*, the Czerny-Lembert suture is justly criticised, "as it requires a useless expenditure of time," etc. On page 713, under *Gastrotomy*, we find "the visceral wound is closed in the usual manner with Czerny-Lembert sutures." A *Perforated gastric ulcer* is to be excised and the resulting wound closed; whereas many successful cases are reported, where the much simpler procedure of invaginating the ulcer with Lembert sutures was followed. In *Pylorectomy* the old operation of uniting the duodenum to the cut end of the stomach is carefully described. (!) In cases of *Perforated typhoid ulcer* the surgeon is warned not to operate "during early shock." Is that wise advice? Finally, the index refers the reader to page 48, instead of page 494, for *Krönlein's Method*, while it contains no reference at all to the paragraph describing *Perforated typhoid ulcer*—mistakes which should not have occurred in a third edition.

There are 100 pages on operative surgery, which add some-

what to the value of the book. Some of the plates throughout the book are beautiful, and the work abounds in good illustrations and diagrams.

"Very good, but unequal," will, we think, be the verdict of most.

Introduction to the Outlines of the Principles of Differential Diagnosis, with Clinical Memoranda. By FRED. J. SMITH, M.A., M.D.Oxon., F.R.C.P.Lond., Physician (with care of out-patients) and Senior Pathologist to the London Hospital. Pp. 353, 8vo, 1 plate. London: Macmillan & Co., Ltd. 1899. Price 7s. 6d. net.

THIS is a very interesting and valuable book, but it is one which will be of use rather to the house physician, practitioner and teacher than to the student, for the reason that it largely presupposes the knowledge that is to be gained from ordinary text-books of clinical diagnosis, and also requires a very considerable amount of general medical knowledge for its understanding.

To take all its faults first: the arrangement is chaotic, the index is badly done, and there are great omissions. For instance, not a word is said about the examination of the blood, and such not unimportant organs as the liver and spleen are merely mentioned casually here and there. The subjects treated are dealt with in a very unequal way; some have much space given to them; others, equally important, are passed over in a few lines or paragraphs; and, as is probably inevitable in such a work, there are occasional errors in statement.

On the other hand, if one is content to let Dr Smith talk only of the things he wants to tell us about, there is abundant information, not always new, but very often put in new ways. There is constant evidence that the author is a careful and painstaking observer, a man who thinks and who is not afraid to state his dissent from generally accepted views.

One very useful feature of the book is the large number of tables or parallel statements for use in differential diagnosis. These have been very carefully drawn up, and are unusually accurate. Some of the best are to be found in the sections on infectious diseases and on the urine. The last chapter, on urgency cases, is the best of the kind, in short compass, that we have seen.

The Hygiene of Transmissible Diseases. By A. C. ABBOTT, M.D., Professor of Hygiene, University of Pennsylvania. Pp. 311. Illustrated. London: Rebman, Ltd. 1899. Price 14s.

THIS book deals mainly with the epidermology and prophylaxis of the various organismal diseases. The dissemination of typhoid fever and of cholera is graphically illustrated by means of maps, and the charts showing the incidence of and mortality from these diseases in relation to water-filtration are very striking. But amongst the prophylactic measures against these diseases and others, such as plague, we find no mention of the value of immunising vaccines; a method, no doubt, still on its trial, but one in which all published statistics are favourable. We are pleased to find, however, that the chapter on Malaria is up-to-date, for that is a subject on which our knowledge, owing to so much recent work, has advanced by leaps and bounds—knowledge too which can be applied in the actual prevention of that ever-present scourge and chief factor in tropical pathology.

A very good chapter is that devoted to chemical disinfection and disinfection by heat, and here again we find the results of recent valuable work recorded. So far as it goes, the book is a most useful one, and we have pleasure in recommending it.

Recent Literature

CRITICAL SUMMARIES AND ABSTRACTS

SURGERY

GENITO-URINARY SURGERY

By JOHN SHAW M'LAREN, M.A., F.R.C.S.Ed.,
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The Incision-Cystoscope, a new instrument for performing Bottini's operation on the Prostate.—In an interesting digest, published in a previous number of this journal, Mr Cathcart discussed the operative measures for treatment of enlarged prostate, mainly, however, with regard to the position which castration had attained in the opinion of surgeons as a curative procedure. For the cases for which catheterism does not suffice, castration is probably at present the operation most frequently performed, more frequently even than prostatectomy. Surgeons, however, are ready to confess that they cannot always predict success nor exclude unpleasant results with the certainty that was at first anticipated. In fact most of the operative procedures are still more or less on their trial, and it becomes us

to receive and entertain respectfully any suggestion for the improvement even of such as may for the time have taken a secondary place. Amongst those which have at this moment few ardent supporters in this country is the operation known as Bottini's, and it is perhaps scarcely to be wondered at that surgeons hesitate to favour it. The instrument used in it resembles, to a certain extent, a lithotrite in which the male blade is represented by a galvano-cautery knife which can be moved backwards and forwards in the instrument. It is introduced into the bladder, the beak is turned downwards, the current is switched on, and the knife is made to burn its way for the desired distance into the prostate.

It is some time since Bottini reported over fifty complete cures in eighty cases, and it is not only the inventor of the method who has published successes. British surgeons have not taken kindly to it; Hurry Fenwick failed to meet with any success; but in America and on the Continent the operation is performed.

Guiteras (*A report of twelve cases of prostatic hypertrophy benefited by the Bottini operation, New York Academy of Medicine*, 9th May 1899) reports a favourable result in twelve cases. Eucain anæsthesia was tried, but in most cases ether was required. He considers the operation advisable in cases of marked irritability of the bladder, especially when the amount of residual urine is increasing, and where bleeding and cystitis follow catheterisation—in fact in cases where most would agree an operation of some kind is indicated. The earlier the operation is done the better the result.

Professor Frisch (*Ueber Bottinische galvanokaustische Incision der hypertrophirten Prostata, Wiener Klinische Wochenschrift*, No. 48, 1898) has an interesting paper on the subject. He points out how little favour the operation received for many years, till Freudenberg by his strenuous advocacy revived interest in it. He gives full details of ten cases operated on during a period of four months. Eight were cases of complete chronic retention, two of incomplete retention. There was a complete cure in four cases, permanent improvement in one, and temporary benefit in one; in three there was no improvement, and one patient died as a result of the operation. The published cases give fifty per cent. of cures, and a mortality of seven per cent. But on a closer analysis, it becomes evident that the improvement which was recorded in many cases can scarcely without reservation be ascribed to the operation, while in others it was merely temporary, or else the cases were not observed for a sufficient period. One must be especially careful before accepting the report of improvement in cases of incomplete retention. Frisch evidently thinks that many unfavourable cases have been left unrecorded; he hints too that Bottini may have selected his

cases with care, since he can record only eighty cases in about a quarter of a century—certainly rather a contrast with Frisch's ten cases in six months. A fully satisfactory result is to be expected only in cases of complete permanent retention, with a bladder which retains its power of contraction.

There can be no doubt that in many cases the results have been highly successful, and if one chief disadvantage could be removed, such might be still more frequent. As Wossidlo says (*Centralblatt für die Krankheiten der Harn-und Sexual-Organe*, Band xi. Heft 3), in spite of the favourable result in most cases, and the comparative simplicity of the technique, there is one defect of the method which every one who has adopted it must have suffered from—the operator works in the dark. The knowledge that one is cutting with a red-hot knife in the bladder without seeing what is going on produces a most uncomfortable feeling of uncertainty, and the fear of disastrous results is never absent. Wossidlo has devised an instrument which combines the Bottini "Incisor" with the cystoscope, so as to have the operation under the control of sight. The carrying out of the technical details presented many difficulties; the optical apparatus, the cautery-knife, and the channel for the stream of water to keep the instrument cool, had to be combined in the smallest possible circumference, while the insulation of the knife with its strong current had to be provided for. He calls the instrument the Incision-cystoscope. It consists of a cystoscope, at the side of which the Bottini knife lies. It runs in a groove on the surface of the instrument, where it is curved like a catheter with a short beak, and by means of a screw it can be made to move backwards and forwards into and out of the groove in the beak; when it has been drawn out of the beak —as it would be in incising the prostate—the instrument resembles in appearance a lithotrite with its blades opened, the knife occupying the position of the male blade. The depth of the incision can be regulated by a centimetre scale affixed to the screw. In the groove beside the knife the cystoscope lies; it can be removed at will, or fixed in its place by a screw. The knife, of course, has a separate battery of its own. There is the usual channel for the stream of water which keeps the instrument cool. As the knife and the cystoscope lie close together, it is possible with this instrument to see the movements of the knife within the bladder, and to control the direction of the incision.

After the operator, with the aid of the cystoscope, has fixed upon the part to be incised, he moves the screw, the knife appears in the field of vision, and as soon as it touches the prostate, the current is turned on and the knife is seen to glow. As the screw is still further withdrawn, the knife disappears from sight as it slowly cuts its way into the gland. When the incision

is the requisite depth, the knife is made to emerge from the prostate, and after the current has been turned off, to sink again into its place in the beak. The appearance of the incision can then be observed through the cystoscope. Should bleeding occur, the cystoscope part of the instrument can be withdrawn from the groove, and while it is being cleaned a catheter may be passed down the groove where the cystoscope was, and the bladder can be washed out. Wossidlo operated on one case on the 24th of January, and was satisfied with the instrument. But he is silent as to the result, though the paper is published six weeks later. The instrument is certainly an ingenious one, and it will be interesting to watch whether it brings Bottini's operation into greater favour.

Movable Kidney.—It is mainly the operative treatment for this condition that still excites interest in the minds of surgeons, but some points with regard to symptoms and differential diagnosis as discussed in a paper by Deaver (*Annals of Surgery*, June 1899) are worth noting before examining the new operation which Deaver commends.

The best position of the patient in which to detect and replace a movable kidney, is lying upon the side opposite to that of the organ supposed to be affected. The legs should be flexed upon the thighs, the thighs strongly flexed upon the abdomen, and the spinal column flexed so as to bend the trunk as far forward as possible. Displacement of the organ can easily be detected if the patient now takes one or two deep inspirations—a movable tumour characteristic in shape, which can be restored by pressure to its normal position. Throbbing of the aorta is frequently felt and seen, and there are the well-known sensations of dragging, discomfort and pain. In the majority of cases there is neurasthenia with dyspeptic derangement; but when the organ has a wide range of movement there may be sudden excruciating abdominal pain, with nausea, vomiting, and in some cases fever, and associated with this there may be marked rigidity and tenderness of the abdominal wall over the displaced organ; the pain is increased by assuming the erect posture. Frequent urination is common in these cases. In a few instances hæmaturia has been noted.

From the foregoing description it will be obvious that in the absence of acute symptoms the condition cannot easily be confounded with any other disease, and there is little need to consider those from which the author takes the trouble to diagnose it; but it is equally plain that "acute" cases might be mistaken for appendicitis or for renal colic. The sudden onset, the vomiting, the pain in the lower part of the abdomen, the rigidity of the abdominal wall in the same area, may each, or all, be found both in

appendicitis and in movable kidney; while careful observers must have noted that pain in the testicle—another symptom of movable kidney—sometimes accompanies appendicitis. Consideration of the following points will help. Appendicitis is much commoner in men; it often follows some indirect violence, or the taking of indigestible food; the site of the pain in it does not shift, the area of rigidity is more circumscribed, raised temperature and pulse-rate are common. Movable kidney is much commoner in women (often neurasthenic); the site of the pain may shift, slight pressure does not cause such acute pain, urination is often disturbed. Movable kidney, attended by both pain and rigidity, requires very delicate manipulation to detect it; if it cannot be felt slipping away under the examining finger anæsthesia may be required. The author had been called to operate for appendicitis in a case which, under anæsthesia, turned out to be one of movable kidney, in a nervous individual with symptoms of acute indigestion. He had frequently seen the mistake made. It is well to remember Frank's "rule" for distinguishing between abnormal and normal movement of the kidney (*Genito-Urinary Surgery*, White & Martin, p. 771). With the patient lying on the back, the surgeon standing on the right side, places the four fingers of his left hand beneath the hollow of the loin below the twelfth rib. The thumb in front encircles the abdomen just below the costal arch, but without exercising any pressure. The patient is then directed to draw a full breath. Immediately before expiration, the surgeon begins to grasp the loin, and follows the liver as it recedes up under the ribs. If with the right hand a kidney can be felt lying entirely below the grasp of the left hand this organ is *pathologically* movable. If the right hand presses on this tumour whilst the left hand relaxes its grasp gradually, the tumour can be felt to slip suddenly between the fingers and thumb, and to disappear upward. On the other hand, a kidney which descends so that only its lower half can be felt, but which moves back to its place on expiration, is *physiologically* movable.

Reports of cases where the mistake has occurred show the need for care in distinguishing between movable kidney and "renal colic." When to the other similar symptoms hæmaturia is added in some cases of movable kidney, the diagnosis may not be easy till anæsthesia is produced. A properly taken X-ray picture will help in the detection of a stone.

As was said before, it is the treatment of the condition which mainly excites interest in the minds of surgeons. Deaver does not attach the same value to "palliative" treatment as most practitioners do, in fact he thinks appliances are more harmful

than useful; the ordinary operative procedures he discusses and condemns, and it must be confessed with some reason.

When Hahn first performed the operation of nephrorraphy twenty years ago, he passed the anchoring sutures merely through the fatty capsule; this of course failed to retain the kidney in position. Since then various modifications have been adopted; the sutures were made to include the true capsule; then with increasing boldness they were passed well into the kidney substance; later still the capsule was incised and peeled off for a short distance on each side, the capsule flaps alone receiving the sutures; and lastly, the absurd ingenuity of Vulliet devised a method by which the patient himself supplied the suturing material in the shape of a strip of tendon from the spinalis dorsi. Now, each of these modifications implies dissatisfaction with its predecessor. The objections are mainly two —relapse, and untoward results. As regards relapse, most surgeons admit that the result is uncertain; the deformity often recurs. Experiments seem to prove that sutures passed through the renal parenchyma are very quickly disintegrated, so that the kidney is apt soon to fall out of its place. Deaver lays stress on this point, and asserts further that if the kidney be held up by the ends of a suture, the weight of the organ is sufficient to cut the sutures out. He contends that in the "few instances" of cure the success is due to the inflammation and granulation set up. He mentions having seen such a practised surgeon as Hayes Agnew removing a movable kidney on account of recurrence of symptoms for which six months previously he had performed nephrorraphy. As regards "disastrous results" following the passing of sutures through the organ, he has seen urinary infiltration and abscess of the kidney necessitating nephrectomy; and he states that Christopher Heath has discarded the operation of suturing on account of the injurious effects of urinary extravasations. The operation recommended by the author consists in anchoring the kidney in position by gauze strips passed under it. He states that it was suggested to him by Senn. The writer had the pleasure of assisting Dr P. H. Maclaren on a recent occasion when he performed it. It seems a rational procedure.

An assistant presses up the kidney into position. A "vertical" incision is carried through the loin-space. The fatty capsule is cut away as far as the hilus, the organ, in order to do this, being entirely delivered, if required. The capsule proper of the kidney is next carefully scarified with the knife, without trespassing upon the kidney substance. Two long pieces of gauze are passed, one under each pole of the kidney, and the organ is replaced. With the organ *in situ*, and the pieces of

gauze carried beneath the poles protruding from the wound, more gauze is gently packed around and over the kidney, between it and the pieces first introduced. Over the gauze packing the long strips are tied, and a large gauze pad is placed upon the anterior abdominal wall over the site of the kidney. The wound is dressed, and the dressing and anterior gauze pad are securely held in place by a strip of adhesive plaster. Additional dressing is applied and retained by a bandage. A quarter of an hour is required for the performance of the operation. The gauze is removed at the end of a week or ten days, and the bed of the kidney and the surface of the organ will be found to be studded with healthy granulation. The wound is gently repacked and dressed in the usual way; it heals completely in from four to five weeks. Reports are given of nine cases operated on during a period of eighteen months; the result in each case was said to be a most satisfactory cure.

OBSTETRICS

By J. W. BALLANTYNE, M.D., F.R.C.P.Ed.,
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The Progress of Ante-natal Pathology.—When the birth-rate begins to go down, the value, economic as well as sentimental, of the unborn infant begins to go up. When few infants are being born it becomes important that they shall come living to the light at the full term, well formed and healthily capable of independent extra-uterine existence. The falling birth-rate is reflected in current medical literature by what may be called a "boom" in articles dealing with fœtal diseases and deformities, with ante-natal diagnosis, with puericulture, and (although the connection is not at first sight evident) with the medical restriction of marriage. This rapid increase in contributions on ante-natal morbid states has been specially marked in the journals of France and Italy; for instance, in the volume of the *Annali di ostetricia e ginecologia* for 1899 there appeared thirty-five original articles, and of these fifteen were directly concerned with fœtal pathology and physiology; and in the recently published part of *L'Obstétrique* (March 1900) there are three contributions, all of which have to do with the unborn infant, one being on the passage of alcohol from mother to fœtus, another dealing with hydramnios and twins, and the third with the radiography of the fœtus *in utero*. Further, this same appreciation in the value of fœtal life is shown in the growing popularity of the obstetric operations,

such as Cæsarian section and symphysiotomy, which give both mother and child a chance of surviving, and in the coincident and consequent disapproval of the so-called fœticidal procedures such as craniotomy (on the living fœtus) and artificial premature labour. The foregoing statements may all be established and illustrated from current medical literature.

With regard to the **checking of fœticide** and the consequent stoppage of one cause of a diminishing population, Pinard (*Ann. de gynéc.*, lii. p. 81, 1899) deals at some length in an article on Cæsarean section, symphysiotomy, craniotomy, and the induction of premature labour. He states that it is his firm conviction that in the treatment of pelvic contractions the following operations must disappear:—(1) artificial premature labour; (2) all operations, whether by forceps or version, which entail the crushing of the fœtal head against resisting osseous structures, either at the pelvic brim, or in the cavity, or at the outlet; and (3) embryotomy on the living infant. On the other hand, the treatment of pelvic contractions must, according to Pinard, include: (1) the temporary enlargement of the pelvis by symphysiotomy, pubiotomy, and coccygotomy; (2) the conservative Cæsarean section, or Cæsarean section followed by total or partial hysterectomy; and (3) embryotomy on the dead infant. It may be doubted whether the profession in this country is prepared to endorse Pinard's somewhat sweeping propagandism; but it is nevertheless a fact that obstetricians everywhere are beginning to ask themselves whether they ought not, in every case of labour where the life of the fœtus is endangered, in the first place to make known to the parents the alternative plans with their relative maternal and fœtal risks. A stumbling-block in the way of the popularisation of the Cæsarean section and of symphysiotomy in British obstetric practice at any rate, is no doubt the external incision; and until obstetricians very strongly recommend these operations, the public will be very slow to respond; statistics may be multiplied showing how almost free from risk it is to have the abdominal cavity opened into aseptically, but it will be long before the popular mind frees itself from the haunting dread of such operations. It may be objected that Pinard goes too far in counselling the abandonment of forceps and version when these operations entail squeezing of the fœtal head, but of this there can be no doubt, that a difficult forceps or version case often resolves itself into an embryulcia in all but name. With regard to the condemnation of artificial premature labour, so typically the British operation, there are probably few who would agree with Pinard; at the same time, it will be well that we all keep clearly in mind the risks that the fœtus runs

in being brought into an extra-uterine environment before the proper time, and that we take every means to make an artificial environment for him which shall closely resemble the intra-uterine one (*e.g.* by means of a suitable couveuse, etc.).

After all, the question of so-called **therapeutic fœticide** comes to depend largely upon the view we take of our power to ordain that the fœtus shall die. To this subject Pinard returns in a recent number of the *Annales de gynécologie* (vol. liii., Jan. 1900). He puts to himself the following question: "A woman in labour cannot be delivered spontaneously on account of contraction of the pelvis; the fœtus is at full term and alive; the interference that you regard as necessary, indispensable, and indicated, is forbidden to you by the patient herself or by her relatives; another means of treatment is proposed to you, and it implies the death of the infant; under these circumstances, what are you to do?" Pinard examines in turn the paternal right to decree the death of his offspring, the maternal right to demand the same thing, the power that the medical attendant has during a confinement to decide whether the infant shall live or die, and the right of anyone else (*e.g.* the managers of the Maternity Hospital) to interfere in the matter. It is unnecessary to detail the arguments *pro* and *con* which Pinard brings forward, the conclusion to which he comes is that the right of life and death over the infant belongs to no one, neither to the father nor to the mother, nor to the medical attendant, nor even to the director of the hospital. The infant's right to his life is an imprescriptible and sacred right which no power can take from him. The right of choosing the operation to be employed belongs to the medical attendant alone. It would seem that this conclusion takes us further than most of us would care to go, for it would or might entail the forcible giving of chloroform and the performing upon the anæsthetised mother of an operation to which she had objected. It is possible that under the circumstances even Monsieur le Fœtus might be imagined to be unwilling to demand such a procedure! When it can be proved that Cæsarean section and symphysiotomy have a maternal mortality in general as well as in hospital practice which is no higher than the maternal mortality after craniotomy, it is more than likely that what Pinard would force upon the public will come about naturally by a growth of intelligent opinion. In the meantime his statistics are striking: maternal mortality after embryotomy on the dead fœtus, 11·5 per cent.; maternal mortality after Cæsarean section (Bar), 6·4 per cent.; maternal mortality after symphysiotomy, 12 per cent.

One of the impediments in the way of an increase in our knowledge of ante-natal pathology has always been the difficulty

of diagnosing intra-uterine morbid conditions. The hope that was freely expressed that the Röntgen rays might irradiate this dark part of the field of medical diagnosis has not been thus far fulfilled. Bouchacourt, in two articles (*L'Obstétrique*, Ann. 5, pp. 20, 137, Jan., Mar., 1900), endeavours to show how it is that **intra-uterine fœtography** has not been successful. Great things were expected of it: by the profession, the diagnosis of presentations and positions and fœtal monstrosities; by the public, the ante-natal diagnosis of the sex of the unborn infant. Small things have been obtained from it: to quote from Bouchacourt, it has been found possible by means of the Röntgen rays to discover the presence of a head in the pelvic cavity or of a portion of the fœtal skeleton in the maternal abdomen when the woman was thin, and just in the cases where, therefore, the ordinary means of exploration would place these structures within the grasp of the examining obstetrician. The obstacles to be overcome have been the movements of the fœtus excited by the influence of the Röntgen rays (a fact which Bouchacourt regards as incontestable), the opacity of the pelvi-vertebral skeleton of the mother and the darkness of its shadow, the presence of the liquor amnii, the living uterine wall and placental tissue, the respiratory movements of the mother who breathes very deeply on account of fright (!) and the difficulty of bringing the sensitive plate equally near to the different parts of the gravid uterus. Some of these difficulties may yet be overcome; others would appear to be almost insuperable. Bouchacourt quotes largely from the experiments of Varnier.

Gradually information is accumulating, derived both from experiment and from clinical experience, regarding **the passage of toxines, microbes, and poisons from the mother to the fœtus.** A noteworthy addition to the literature of this subject is that by Maurice Nicloux (*L'Obstétrique*, Ann. 5, p. 97, March 1900), who describes in detail the methods and apparatus by means of which he investigated the passage of alcohol from mother to fœtus both in the case of animals and in the human subject. It is unnecessary here to enter into the methods and means employed, but it may be stated that they seem to do away with the most evident sources of fallacy. The animal chosen for experiment was the guinea-pig (in one instance the bitch), and a 10 per cent. solution of alcohol was introduced into the stomach by an œsophageal tube. About an hour afterwards the animal was killed and the blood from the carotid artery collected; then the uterus was opened and the fœtuses removed: if they were near term and if the quantity of alcohol introduced had been large, blood was taken from their carotids; if, on the other hand, they were very small, and the quantity injected very small,

their organs and tissues were minced up; finally the amount of alcohol in the maternal blood and liver and in the fœtal blood and tissues was estimated and compared. The results obtained were very striking: in all the cases the alcohol had passed freely from the mother animal to the fœtuses, the quantity in the two bloods being nearly equal; if the quantity injected was too small to be detected in the fœtal blood it was noted that the amount in the minced fœtal tissues was nearly as great as that in the maternal liver; however small was the dose of alcohol injected (½ c.c. per kilogramme, for instance), it was yet sufficient to cause the appearance of alcohol in the fœtal tissues. In six cases Nicloux gave alcohol to parturient women about one hour before the delivery of the infant, and estimated the amount of it which was found in the blood of the umbilical cord. The observations on the human subject gave similar results to those obtained from the guinea-pig and dog. The author holds that these experiments prove the reality of the passage of alcohol from mother to fœtus, and the possibility of the intoxication of the latter. To the condition produced it is proposed to give the name, "congenital alcoholism," but it may be suggested that **fœtal alcoholism** would be more suitable, and that the name congenital alcoholism is already given to another slightly different state. Nicloux asks what dire results may be expected from the action of alcohol upon the developing organism and more particularly upon the developing nervous system in such cases? The answer is found in the teratogenic experiments of Féré and others: in the early stages of ante-natal life monstrosities and non-developments will result; in the later stages of fœtal life it may be expected, from the clinical observations of Sullivan on the maternal inebriety (*Journ. of Ment. Sc.*, July 1899), that still-birth, congenital debility, and morti-natality will be the consequences of the passage of alcohol to the unborn infant.

Another line along which ante-natal pathology has made and doubtless will make progress is in the direction of **puericulture** or the treatment of the pregnant woman with the view of benefiting the fœtus *in utero*. A leading article in the *British Medical Journal* (March 24, 1900, p. 718) draws attention to the fact that in various continental countries women of the working classes are either prohibited by law from laborious physical toil both before and after confinement (six weeks before to six weeks after), or have at least the opportunity given to them of entering a resting home for pregnant women (Paris possesses three such hospitals) for some weeks ante-partum. Statistics show in a most striking manner the benefit to the fœtus as exhibited by greater weight and strength which follows when the mother

rests from her work during the later weeks of gestation. In the absence of such homes in this country the physicians in charge of maternity hospitals and the managers of these institutions ought to set apart beds and accommodation for pregnant women, and especially for women whose pregnancies show evidence of pathological developments.

The **regulation of marriage by law**, with the intention of thus preventing the union of the unfit and the consequent bringing into the world of progeny also unfit, has been exciting a good deal of attention in the United States, both in the general press and also in medical societies and journals. The Parker Bill failed to pass the Ohio Legislature in 1898, and the Hon. C. W. Parker, in an article entitled "Legislative Problems in the Regulation of Marriages" (*Jour. of the Amer. Med. Assoc.*, xxxiv., p. 521, 1900), gives his explanation of the cause of the failure. In the first place there was the public sentiment against legal restriction of marriage (and, he might almost have added, of everything else), founded upon conservativeness, mock modesty, and ignorance. The physical examination of the female candidates for marriage is viewed by most men and women with abhorrence; Parker says, "by some men and some women who know nothing of the God-given possibilities involved in an educated maternity." In the second place, there were the purely legislative difficulties, such as log-rolling, political enmity, responsibility to constituents, personal unfitness for matrimony of some of the legislators themselves (!), and jealousy of other and more advanced legislators. In the third place, there was the ignorance of what an "examination" really meant. In order to get rid of these obstacles and difficulties Parker is willing very considerably to modify his bill. He will exempt, at first, the female candidate from examination, for, he says, men do not wish to marry a diseased mate. He will make the restriction apply only to persons proven to be dipsomaniacs, shown to be actively tubercular (not predisposed merely), or actually imbecile, etc. In the third place, there must be education of legislators, of all men, of all women, of the classes, of the masses, as to the results of marriages between the unfit. Albert H. Burr (*Jour. of the Amer. Med. Assoc.* xxxiv., p. 524, 1900) contrasts the care taken with stirpiculture and the breeding of domestic animals with the absence of all restrictions of human unions. One quarter of the human race die before the end of the first year of life, "and the largest factor in this is defective parentage." There are already some restrictions, such as minority, consanguinity, and, in the Southern States of the Union, miscegenation; but there ought to be medical restrictions also, such as the existence of infective venereal disease, epilepsy, habitual

drunkenness, hereditary insanity, and tuberculosis. D. R. Brower (*Jour. of the Amer. Med. Assoc.*, xxxiv., p. 523, 1900) and Mrs Alice Lee Moqué (*Ibid.*, p. 526) have articles advocating the same views. The medico-legal restriction of marriage is, however, a very difficult problem. Education must be the first thing attended to. When the causes of fœtal disability and death which act through the parents come to be more widely known, then perhaps the marriage of individuals with no capital of health will be publicly regarded in the same way as the union of individuals with no capital of wealth.

OPHTHALMOLOGY

By ANGUS M'GILLIVRAY, C.M., M.D., F.R.S.Ed.,
Ophthalmic Surgeon, Dundee Royal Infirmary; Surgeon, Dundee Eye Institution, etc.

Herpes Zoster Ophthalmicus (by Dr Abadie of Paris, *Arch. d'Ophtalmologie*, May 1899).—Regarding the etiology of herpes zoster Abadie discountenances the commonly accepted views, namely that of a trophic lesion due to changes in the sensory nerves, and Brissaud's spinal theory in which the eruption involves an area of skin innervated by a segment of the spinal cord.

The new theory propounded by Abadie is that herpes zoster is due to a pathological condition of the arterioles and the vasomotor nerves in the cutaneous area over which the eruption appears. As a rule the arterial supply to the skin follows the same course as the sensory nerves, and this is the reason why the eruption in herpes, occupying really the vascular territory, has been considered as occupying the nervous territory. In herpes zoster ophthalmicus, if the affection be an inflammation of the trigeminus, how are we to explain why only one of its divisions (and always the same division) should exhibit cutaneous lesions? Why do the superior and inferior maxillary branches escape, why should the skin supplied by them never be invaded by the eruption? Abadie maintains that these anomalies are explained by his theory. According to it the skin lesions should appear in the area supplied by the supra-orbital, frontal and (sometimes) nasal arteries, and this he says is what actually occurs. Accompanying the intra-cranial part of the trigeminus trunks are filiments of the sympathetic system which come from the carotid plexus, and are derived from the superior cervical ganglion. They extend along the branch of the ophthalmic artery to their termination. Abadie argues that if a morbid inflammatory process affects the trigeminus, or the

Gasserian ganglion, it must involve the sympathetic filiments accompanying the nerve trunk; inflammatory irritation of these vaso-motor nerves will be followed by changes in the arterioles, of which they regulate the calibre. This vaso-motor disturbance is, according to the author, sufficient to explain both the seat of the eruption, which does not accurately follow the area of nerve supply, but the vascular area, and the trophic lesions of herpes zoster ophthalmicus.

Abadie finds further support for his view in the fact that herpes zoster of the thorax does not affect the first and second intercostal spaces. The arteries of these spaces come from the subclavian, whereas the arteries to the remaining spaces are derived from the aorta.

As regards treatment the author states that the most efficacious remedy in the ophthalmic variety of herpes zoster is quinine, a drug which is essentially vaso-constrictor in its action on the sympathetic.

Sterilised Ophthalmic Dressings (by Chetwood-Aiken, *Ophthalmic Review*, Jan. 1900).—A number of circular pads of absorbent tissue are cut and placed in a tin, each pad being separated from its fellow by a leaf of paper. To these is added a tray containing "swabs" or sponges made of small rolls of lint, the whole being then placed in a suitable steam sterilizer. Here a pressure of 20 lbs. and a temperature of 120° C. to 130° C. is produced, and maintained for a sufficient time to ensure absolute sterilization. The tins are then removed and hermetically sealed.

The form of tin employed is similar to that used by Messrs Wills & Co. for putting up tobacco and cigarettes, and is opened in a like manner by a cutter in the cover. Each tin contains twenty pads and a sufficient number of swabs for an operation. By this means the surgeon procures his dressings ready cut, sterilized and packed—a great convenience in emergency operations. The pads retain their shape and do not adhere to one another, and each can be removed singly without touching the remainder. The tins cost 18s. per doz., and are made by Messrs Ferris & Co. of Bristol.

Bactericidal Action of Tears (by Valude of Paris, *Annales d'Oculistique*, Sept. 1899).—Valude has been fortunate in securing a sufficient supply of tears from a healthy conjunctival sac, which he was able to procure under strict antiseptic precautions. The tears were colourless. translucent, with an opalescent appearance as if some very thin flakes were in suspension, and slightly alkaline. The fluid became clear on standing, the

flakes collecting at the bottom. Chemical examination showed that the liquid was undoubtedly normal lachrymal secretion, and its sterility was proved by the fact that broth remained clear and without a trace of culture fifteen days after receiving a drop of the fluid. Further, the pipettes containing the original fluid were placed for fourteen days in the incubator at 30° C., and none were changed. From a series of experiments with anthrax bacillus, Valude arrives at the following conclusions:—the b. anthracis cannot develop in the lachrymal fluid; the adult bacilli, on being plunged into the lachrymal fluid for some days, lose their power of growing in nutritive broth and become non-pathogenic for guinea-pigs. The same results, practically, were found with regard to the b. coli and the staphylococcus aureus and albus.

The Operative Treatment of High Myopia (by Rogman, *Annales d'Oculistique*).—The author reports twenty-one cases of removal of the lens in high degrees of myopia. In the performance of the operation he makes a preliminary discission, exercising great care not to wound the hyaloid membrane of the vitreous, and to avoid the escape of any of the aqueous humour. The lens is removed several weeks later by Teale's suction operation. Rogman claims that his results are most excellent, only one being lost as the result of detachment of the retina eight months after the operation. The difference in refraction of the eye produced by the operation ranged between sixteen and twenty-six dioptres. With but one exception the acuity of vision without correcting lenses was better after the operation than it was before the operation with glasses, the degree of vision with glasses after the operation ranging from 5/10 to 5/30.

Colloidal Silver (Collargolum) in Gonorrhœal Ophthalmia (*Ophthalmic Record*, Decr. 1899).—Schlossmann, from an extensive experience with colloidal silver as a local application in gonorrhœal ophthalmia, concludes that it is the best remedy at our disposal in the treatment of this obstinate affection. He recommends a 1 to 2 per cent. albuminous solution, to be applied to the conjunctiva with a camel's hair brush. The instillations, he states, are entirely painless. Improvement usually began with the first application, and in some instances the children were discharged cured after four or five days.

Collargolum is the name given to an allotropic form of silver, which is said to be non-poisonous, and which is permanently soluble in water and albuminous fluids. It has been used somewhat extensively during the past year in the form

of an ointment (unguentum Credé) as an inunction in various septic and inflammatory affections.

The Treatment of Trachoma by the Excision of the Conjunctival Cul-de-sac (by Galezowskie, *Recueil d'Ophtalmologie*).—This form of treatment has been found useful in cases which have resisted all other measures. Three important points: (1) the excised portion must include only the conjunctiva; (2) the incision must be prolonged as far as possible to the angle of the eye; (3) it must not include the tarsus. After cocainisation or general anæsthesia, the upper lid is everted and one of the arms of the author's double forceps is introduced beneath it; the conjunctiva is seized and drawn away from the tarsus. It is then dissected back the whole length of the eyelid, then drawn forward, the portion held by the forceps cut off and the eye closed. An ice-bag is applied and a carbolised collyrium used, cicatrisation resulting in a few days.

Notes on Foods, Drugs, &c.

The Bayer Company have forwarded us samples of several of their preparations. They have certainly been fortunate in being the discoverers of many synthetic preparations which seem destined to occupy a permanent place in medicine. Trional, Sulphonal, Phenacetine, and Piperazine have all proved their value.

Of their newer preparations Salophen promises to be a useful substitute for some of the older salicin preparations, and we have used it with good effect. Protargol is a most valuable addition to our local appliances. Originally introduced by Neisser as a remedy for gonorrhœa, it has been found by many oculists to be more useful than other preparations of silver; it is a useful astringent in catarrh of the nasopharynx, and we have recently used it with success in a case of œdematous eczema which was benefiting but little under other apparently suitable treatment. Heroin has already been referred to (Jan. No., p. 86), and to Aspirin, one of their latest introductions, we have thought it well to give a more prolonged trial before pronouncing an opinion.

Of Messrs H. C. Baildon & Son's preparations we have only space to notice two. Their Cod Liver Oil Emulsion, which contains 75 per cent. of Cod Liver Oil, is so pleasantly flavoured that one has no difficulty in getting even children to take it willingly. Their Valoid Picis, which they have informed us will in future be known as Liquor Picis owing to the word "Valoid" having been registered by another firm, is one of the most active and unirritating tar preparations procurable.

NOTICE.

All communications to the Editorial Department, including Exchanges and Books for Review, should be addressed to THE EDITOR, Dr W. RUSSELL, 3 *Walker Street, Edinburgh, and all business communications, including Subscriptions, Advertisements, &c., to* THE SUB-EDITOR, Dr NORMAN WALKER, 7 *Manor Place, Edinburgh.*

THE SCOTTISH
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ACUTE INTESTINAL OBSTRUCTION

Part of Two Clinical Lectures delivered in the Royal Infirmary in January and February 1900

By CHARLES W. CATHCART, F.R.C.S.,
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THERE are many ways in which a surgeon undertaking an operation may learn lessons of value from the practice of military commanders. Professor Spence's admirable rule for cases of hernia, "when in doubt operate," was said to have been an adaptation of "when in doubt attack," a maxim which had been the guiding principle of a great Italian general. At present I propose to take advantage of the great interest felt in Mafeking and its hero Colonel Baden-Powell, to emphasise the importance of "scouting" in surgery, by illustrating its adaptation to one important department. Every member of the class should read Colonel Baden-Powell's exceedingly interesting little book on that subject, not only for its own sake, but also for its possible applications to surgery. At some future lecture we may look into the book more closely for surgical lessons, but to-day our subject is acute intestinal obstruction, and we will limit ourselves to the bearing of "scouting" on this.

I do not know how a military man would define "scouting," but after reading the manual on the subject, I see that it consists in finding out as much as you possibly can about your enemy before you attack him; in learning, for instance, what his habits are, what he is likely to do if left alone or if

attacked, what force he is in, what position he has taken up, and so on. In order to do this we are told that the scout must train all his senses. He must observe minutely, and interpret what he observes. He must practice in the time of peace. When on duty he must avail himself of every possible avenue of information, and despise no means which will help towards the desired end, remembering always that his object is not personal honour and glory, but to defeat the enemy and "play the game" that his side may win.

In considering the symptoms and treatment of some cases which have recently been under our care we will find some applications of these principles to clinical surgery and be able to trace the advantage of carrying them out, as well as the disadvantage of not employing them.

We will begin with a case of intestinal obstruction not complicated with peritonitis or injury of the intestine, because it gives us a good opportunity of studying the symptoms due to obstruction of the small intestine pure and simple.

CASE I. Robert S., aged eight, from the Canongate, was admitted into Ward V. on 24th January 1900, on account of vomiting and constipation, which had lasted for three days.

On the 21st inst. the boy was quite well, and had his dinner at one o'clock as usual. The meal consisted of broth. In the afternoon he went to a shop where his friends gave him two saucerfuls of an Italian preparation consisting of macaroni, tomatoes, beans, and salad oil. Later on he had an apple, and before his tea or supper he ate a dish of fried potatoes. His mother was quite unaware of this until after he took ill. About 3 A.M. next morning (22nd January) he came into the kitchen where his mother slept, crying with pain in his abdomen. Then he began to vomit, and kept on vomiting very frequently, both on attempting to swallow anything and irrespective of food. He was seen by a doctor during the course of the day, and was given some sort of stomach powders, and put on boiled milk, with ice to suck to relieve the thirst. The pain in the abdomen was severe at times, but intermittent. On the 23rd he was seen by another doctor at 5 P.M., and was given an enema, while hot fomentations and mustard were applied to the abdomen, and renewed

every twenty minutes. At 10 P.M. he was visited by Dr M'Larty, who advised his friends to bring him to the Infirmary, and he was admitted shortly before midnight. The mother says that the vomit was very green, and had a dreadful smell like "stools." The last occasion on which he vomited before operation was in the waiting-room.

When a child of ten months, patient suffered from diarrhœa, which continued many months. He was then treated in the Out-Patient Department of the Sick Children's Hospital. He had scarlet fever subsequently. At the age of six years he had measles and whooping-cough together, and had scarcely recovered from these when he took pneumonia, and was a month in hospital. Since infancy the patient has been subject to attacks of abdominal pain with diarrhœa, brought on by trifling causes, such as getting his feet wet. His mother usually treated him in these illnesses by restricting his diet to boiled milk, and giving him port wine or brandy. A year ago he had a very bad attack of vomiting, with abdominal pain, at a time when he was suffering from diarrhœa. The vomiting seems to have been slight, but he was laid up for a week at this time. He was treated by poultices and stomach powders, and recovered all right.

Family history.—Unimportant.

Condition at 1 P.M. *on January 25th.*—Pulse 112. Temp. 100°. The boy was in a semi-conscious condition, and could give no proper account of his symptoms. He made no special complaint except during examination of the abdomen, when, as the result of manipulation, spasms came on from time to time, and distinct "ladder patterns," from distended coils of bowel, became visible on the surface. The abdomen as a whole was distinctly distended. The note on percussion was tympanitic in the centre; rather dull in both flanks, and especially on the right side. On palpation there was some feeling of resistance on the right side in the neighbourhood of the ascending colon, but no rigidity nor tenderness of the abdominal wall anywhere. Nothing abnormal could be felt per rectum.

Diagnosis.—Obstruction of bowel; from the persistent vomiting and constipation. It was thought, however, that the history of alternate diarrhœa and constipation with pain,

and the presence of ladder patterns, pointed to a former partial obstruction, which had now become complete. A tubercular lesion was thought probable.

An immediate operation was considered necessary. Half an ounce of brandy was given at 1 P.M., and patient put under chloroform. As there was most dulness and some feeling of resistance in the region of the ascending colon, an oblique incision was made on the right side over the outer edge of the rectus muscle, the highest point of the incision being just below the level of the umbilicus. The peritoneum having been opened, a finger was introduced to explore, and the distended small intestine was with difficulty prevented from protruding; soon, however, collapsed coils of small intestine were felt on the right side. These were pulled out till the junction of collapsed with distended bowel was found. Numerous caseous glands, some becoming calcareous, were present in the mesentery. To one of these glands at the root of the mesentery the bowel was tacked down by an adhesion, and acutely bent on itself. The adhesion was put on the stretch and partly broken through by a dissector and partly cut with scissors. The distended bowel was next clamped above the obstruction between two pairs of forceps and opened longitudinally, a purse-string suture having previously been inserted. The flanged end of a long glass drainage tube was introduced through the opening and temporarily tied in, and the intestine was emptied. Ten or twelve ounces of fœtid fluid, besides gas, escaped, and masses of undigested food, including apple skins, several times blocked the tube and had to be cleared out with a long probe. The opening was then closed by a double layer of Lambert sutures. The bowel having been well washed with a saline solution, was replaced, and the wound closed in the usual way. One-fortieth grain of strychnine was given before patient was removed from the theatre. Hot bottles were put round him, and half an ounce of brandy given per rectum. The foot of bed was raised. Pulse (110) was stronger than before operation. No sickness.

Progress.—25th January 1900, 1 P.M.—Patient doing pretty well, still in a restless state. Has not been sick, and has passed flatus per rectum. Pulse 130. Temperature rising, now 101·7°. Patient makes no effort to retain enemata.

which he is getting, so will be fed by mouth. If symptoms of peritonitis threaten to come on patient will get calomel.

26th.—Patient very ill, has been very sick ; retains nothing ; is becoming very restless, and has to be constantly watched. Eyes examined by Dr Sim, who is unable to say if optic neuritis be present or not as patient wont keep still. To clear out the bowel 3 grs. in all of calomel were administered at separate times on the tongue, followed by an enema. The bowel began to act again, and flatus and large quantities of fæces were passed.

January 27th.—Patient seems a little better to-day, he retains all the food given to him, also the nutrient enemata. He is unable to sleep except when he gets trional. He has not been sick since last night. He does not now complain of pain in the abdomen. The temperature is almost normal.

January 28th.—Patient seems a little better. He rests longer and sleeps about an hour and a half at a time. He takes milk fairly well and has been ordered beef-tea. No signs of peritonitis or inflammation of the bowels. His temperature this morning was 99·2°.

January 29th.—Patient better, temperature just a little above normal. Is quite quiet ; does not roll his head about as before ; has not been sick, and is able to take panopeptone, with which he is being fed. He answers quite sensibly when spoken to. His tongue and lips are covered with large epithelial scales. He says he has no pain, but is inclined to be fretful.

January 30th.—The wound was dressed and was found to be quite satisfactory, no signs of suppuration. The patient is now not so restless, he takes his food well, and gets an enema every day which acts very effectually.

From this time the progress continued to be quite satisfactory, and he was discharged convalescent on the 22nd of February.

The obstruction in this case seems to have been due to undigested substances, especially apple skins, blocking a portion of the small intestine, which had been narrowed by cicatricial contraction and adhesion and sharply bent upon itself.

In contrast with this case of simple obstruction we may next consider one of what seems to have been originally

intestinal accumulation without obstruction. It shows us some of the effects of this necessary consequence of obstruction.

CASE II. John A., aged forty-five, was admitted to Ward V. on January 15th, 1900, complaining of pain, sickness and constipation. On the 12th of January patient felt violent headache and was sick and stayed in bed. On the night of the 14th, when seen by a medical man, the abdomen was found very hard, distended and painful, and there was a good deal of vomiting. The vomit had an intestinal sort of smell, but was not fæcal in character nor sour. On the morning of the 15th the distention was not reduced though he was easier. He had 10 min. Liq. Morph. Hyd. every four hours with Bismuth and Sp. Chloroform. Bowels moved slightly, slimy motion. His previous health has been good. Patient says he had a pain in his right iliac fossa about two months ago, followed by a sensation of something giving way.

When admitted on the afternoon of the 15th, patient complained of no pain. The abdomen was moderately distended, partially tympanitic all over, without tenderness. Liver dulness reached only to the costal margin. The tongue was furred. He had a slight flush on his face. He was quite sensible and could answer questions without difficulty.

An enema of soap and water was given and a large quantity of unformed fæcal matter was evacuated.

Next day, January 16th, the following note was taken in the evening:—Pulse 112, temperature 100·5°. Tongue fairly good. Patient complains chiefly of headache and has been perspiring a good deal to-day. The abdomen is considerably distended but not tender. The note on percussion is rather duller than the distention would have led one to expect; there is nothing to be felt per rectum.

Diagnosis.—Possibly influenza with constipation or some accumulation in the bowels and absorption from them—there is nothing to indicate obstruction.

Treatment.—4 grs. of calomel followed by a seidlitz powder and an enema to-night.

The patient got up through the night without asking for a bed-pan and had a motion which was not seen.

January 17th, 12 noon.—Patient looks flushed, breathes quickly and is very restless. The tongue is exceedingly dry, and patient is very thirsty and somewhat delirious. His temperature rose to over 101° during the night, and is still over 100°. His pulse is 120 and thready. The medicine has been followed by two motions, one of which was seen and which was small. The abdomen is somewhat distended, but there is no localised spot of tenderness.

During the afternoon the patient became very restless, always picking at the clothes and wanting to get up. The pulse varied from 130 to 150 and was thready. There was no cough and no respiratory difficulty. The lungs were not examined. The urine contained a trace of albumen.

In the evening he was quite delirious; the abdomen became hard and resistant, but it was not possible to judge if there was tenderness or not. He grew weaker and about 9.30 P.M. began to vomit, and continued to do so till he died two hours later. The fluid brought up was dark brownish green, and fœtid.

Post-mortem examination.—On opening the peritoneal sac there was marked recent and slightly purulent peritonitis, but no effusion other than simply what was adherent to peritoneum. Intestine distended mostly with fluid. No perforation in any part of alimentary canal. No ulceration of intestine. Very slight catarrhal enteritis, probably not prior in development to the peritonitis. Appendix healthy. Bladder and rectum showed nothing to explain peritonitis. Spleen acutely congested, with small hæmorrhage.

The brain showed acute suppurative leptomeningitis extending over both hemispheres, especially in front, at base, on medulla and pons. No apparent source of infection in the ears or other parts of cranium.

It was only after the man's death that his wife gave us a piece of information which would have helped to establish our diagnosis of fæcal accumulation. It seems that about a fortnight before his fatal illness he had been drinking very heavily. For two days he disappeared from his home, and at the end of that time his wife found him locked up in the police cells for having been drunk and incapable. He had never properly recovered from this bout of drinking.

When examining his abdomen I was struck with the fact that the dulness on percussion was much greater than the appearance of the abdomen would have led one to expect. I could not make out any signs of free fluid in the peritoneal cavity, and although we were correct in diagnosing intestinal accumulation, I confess that I did not fully realise the meaning of the dull note with the appearance of distention until the intestines were exposed at the *post-mortem* examination.

The peritonitis was quite recent and was evidently the result, not the cause, of the intestinal accumulation.

The leptomeningitis seemed also very recent. When he was admitted, the only symptom which might have been due to this disease was the headache, but the rapid development of delirium and coma towards the end was no doubt caused by it, although we had not diagnosed it. We may, I think, fairly consider that it was a result of organisms absorbed from the intestines settling down upon membranes weakened by previous alcoholic congestion. Had it not been for the delirium and rapid failure in strength I might have performed laparotomy in order to empty the loaded bowel. When purgatives fail to act this is the only resource; but apart from his weakness, the delirium seemed to indicate a condition which the evacuation was not likely to relieve, and the post-mortem examination confirmed this view when it showed what that condition was. In death from peritonitis and accompanying accumulation, where there is no other complication, the mind generally remains clear to the end.

This is illustrated by the following case, which also shows the evils of delay.

CASE III. John R., aged twenty-three, was admitted to the side room of Ward V., late in the evening of February 11th, 1900, with symptoms pointing to intestinal obstruction and acute peritonitis.

Previously healthy, he complained first on the 7th February of abdominal pain and headache. It was assigned to influenza, and treated accordingly with hot fomentations and medicine. He vomited occasionally, and became completely constipated up to day of admission.

When examined at 10 P.M., 11th February 1900, the

patient looked pinched and anxious. He had a rapid thready pulse, and rested propped up with pillows and with his knees drawn up. He had an earthy, slightly cyanotic look, and had dark rings round his eyes. There were spasms of additional pain every now and then.

The skin of the abdomen was excoriated from the effects of turpentine fomentations. The abdomen was tense, and from time to time, as the spasms came on, showed distinct patterns of distended bowel. It was tympanitic all over except in the right iliac region. Rectal examination revealed nothing.

The operation was begun about 11 P.M., ether being used, chiefly on account of the patient's depressed condition. Mr Cathcart, with Mr Caird assisting, opened the abdomen in the right iliac fossa, and found the small intestine in the neighbourhood of the opening greatly distended and intensely congested. It was of a purple colour, with a dull, granular look on the surface. A long band of lymph was found constricting the bowel some feet above the ileo-cæcal valve. Below that the coils of small intestine were very much contracted and glued together with recent but firm lymph, which in several places seemed to constrict them. The adhesions were freed and the distended bowel was opened and evacuated by means of a glass tube surrounded by a purse-string suture. This opening was afterwards closed with Lambert sutures, and the bowel and peritoneum thoroughly washed with hot saline solution. The wound was closed in the usual way, and a drainage tube left in the pelvis.

At the close of the operation the patient's condition was so grave that transfusion of 20 oz. of saline solution was performed into the right median basilic vein by Mr Caird, while Mr Cathcart was closing the wound. An enema of brandy was given at once, and heat supplied to lessen the effects of the shock.

The patient never rallied properly. His pulse remained feeble and rapid, and his respirations quick. He suffered greatly from distention and pain in the abdomen. Morphia was injected hypodermically, as he seemed too far gone to expect benefit from purgatives. At 4.30 P.M. on the 12th of February he became very restless and excited, throwing his limbs about. Began to wander a little, his mind having been

up to this more acute than usual. At 4.50 he became calmer, then unconscious, and died at 5.10 P.M.

This seemed to be a desperate case on his admission, *i.e.* on the fourth day after symptoms had begun. The constricting band was not well organised, although sufficiently firm to cause complete obstruction. There was nothing in the previous history to lead one to suspect that such a band might have been present, and it is well to remember this. We have no clue to the date of its formation or to the mode of origin. It may have been in the abdominal cavity for some time before it caused obstruction. The "ladder" pattern showed itself in this case of acute obstruction also, and in spite too of the presence of peritonitis. The latter, however, was almost limited to the pelvis.

One of the most distressing symptoms in this case was sleeplessness. He had not slept for three nights before he came into the Infirmary, and nothing that we could give him produced any effect. His brain seemed to be unnaturally active and acute to within a few minutes of his death.

There was no *post-mortem* examination.

We may next turn to a case where localised peritonitis was the cause of obstruction. This is a very serious form.

CASE IV. Mrs R., aged thirty-four, was admitted to the medical side of Ward V., under Dr G. A. Gibson, on the 6th of February 1900, and transferred almost immediately to the surgical side. She complained of pain in the abdomen, and diarrhœa alternating with constipation.

Since the birth of her last child, about five months ago, the patient has not been well. She has felt weak and tired and unable for her house work. She has also suffered since that time from diarrhœa alternating with constipation, and has been troubled with colicky pains in the abdomen, although not severe enough to call for special attention. A few days ago she was attacked by a severe pain in the abdomen, chiefly situated in the neighbourhood of the ascending colon and ileo-cæcal valve. For three days no fæces were passed, but on the morning of her admission the patient says that the bowels acted at home. She had morphia once or twice before coming in to relieve the pain. Patient took castor oil about a week before coming into

Infirmary. When examined about midday on the 6th of February the patient did not complain of colicky pain in abdomen, but palpation and manipulation of the abdomen set up slight spasms, and the outline of distended coils of bowel was distinctly visible passing over the abdomen. Then she complained of pain. There was no distention of the abdomen, and it moved freely in respiration. The note in the flanks was duller than elsewhere over the abdomen, but even there was not specially dull. Mr Cathcart thought he could make out thickening in the neighbourhood of the ileo-cæcal valve. Patient had a good colour and her pulse was quite good. Temperature was also normal. On account of these features of the case and the history of recent action of the bowels, an operation was not thought immediately necessary. Partial intestinal obstruction was diagnosed. Hot fomentations were ordered for the pain if it should return, and if these were not sufficient, turpentine was to be added. Patient was also to get small doses of morphia and was to be fed mainly by nutrient enemata, getting only small quantities of food by the mouth, chiefly hot water if she complained of thirst. Patient remained *in statu quo* that day, but next morning the abdomen was slightly distended, the bowel patterns were more marked, and patient had a pinched look, so it was decided to operate.

The abdomen was opened by an incision in the middle line just below the umbilicus, and some clear fluid escaped from peritoneal cavity. On passing in the finger, numerous adhesions were found between the coils of small intestine. Between adhesions at one point, a small collection of fœtid pus was opened and sponged out. After further exploration it was found that the obstruction was due to adhesions in the pelvis round a coil of intestine which was covered with lymph and was vascular and soft. After considerable difficulty the adhesions were separated, so as to allow the coil to be lifted up. It was then found that the uterus, ovaries and fallopian tubes were thickened and inflamed, and so matted together that it was impossible to define any of them, except a portion of posterior wall of uterus. No abscess could be found in the tubes or pelvis.

An examination of the affected bowel made it evident that it would be impossible to return it to the peritoneal cavity.

An effort to remove some of the lymph resulted in the tearing of a portion of the muscular coat. A part about 18 inches in length was separated from the mesentery. The lower end having been divided, the affected piece was brought over the edge of the table and through it the distended bowel was evacuated. The piece of bowel was then cut off and the ends united by a double row of silk sutures. The coats were all very friable, and great care had to be taken to prevent tearing. The peritoneal cavity was then well douched out with hot saline solution, and the edges of wound in abdominal wall were united with chromic gut stitches; a glass drainage tube containing a wick of gauze was passed into the pelvis, and another piece of iodoform gauze was left in the neighbourhood of the sutured bowel.

The operation showed that there had been an outbreak of peritonitis leading to such adhesion and matting of the small intestine as to cause obstruction.

At the close of the operation, the patient's pulse was 130 in frequency and very weak. A hypodermic of $\frac{1}{10}$ gr. strychnine was therefore given. The bowels were moved while patient was still on the table and a large quantity of fæcal matter was passed. Patient was put to bed, several hot bottles being put round her; the end of the bed was raised and plenty of warm blankets put on. The arms were wrapped up in Gamgee tissue. The hands were very cold, the face was pale and drawn. The pulse was from 120 to 130 in frequency. An enema of an ounce of whisky with beef-tea was administered at once, and a hypodermic of $\frac{1}{30}$ gr. strychnine was also given.

3 P.M.—Hypodermic of $\frac{1}{6}$ gr. morphine given.

5 P.M.—$\frac{1}{8}$ gr. morphine given.

6 P.M.—Pulse very weak, but not intermitting. The patient has complained greatly of spasms of pain and has been only partially relieved by morphine.

9.30 P.M.—Pulse seemed quite gone almost; patient was very restless, her hands were very cold; 36 ounces of saline solution were infused into patient, after which the pulse could be felt but no more. This operation made no permanent improvement.

10.30 P.M.—Patient cold and pulse less; complains of

being uncomfortable. She became gradually weaker and died at 12 o'clock on the night of the operation.

The pus when examined was found to contain streptococci.

Patient was not examined per rectum before the operation, as it was thought to be a case of tubercular peritonitis.

Post-mortem examination. — When the abdomen was opened, recent fibrinous peritonitis was found gluing together the loops of bowel here and there. Small accumulations of pus were found at various places. There was greenish discoloration of the peritoneum lining pelvis and of the general pelvic contents. Fresh lymph had evidently been thrown out since the operation. The glands in the neighbourhood of the pelvis were infiltrated and hard. The piece of bowel which was first removed at the operation was about 36 inches above the ileo-cæcal valve. The sutured portion was watertight. The small intestine was distended throughout. The lower part of the ileum was intensely congested both above and below the sutures.

All the pelvic contents were very firmly adherent. The anterior surface of the bladder was covered with a greenish fibrinous membrane. The only part of the pelvic organs which could be distinguished was the upper part of the uterus. Anteriorly the uterus was adherent to the bladder, while posteriorly the pouch of Douglas was almost occluded.

As this case was one of obstruction associated with pelvic peritonitis from the streptococcus, the relief of the obstruction was necessarily associated with the risk of peritonitis of a bad type from the opening up of foci in the pelvis. The cause of the obstruction was thought to be tubercular at first, and operation was delayed from the want of severity in the symptoms.

As we began with a case of obstruction of the small intestine, we may finish with one of the obstruction of the great intestine, which was fortunately recognised early, and in which the operative treatment has been successful. I showed you the specimen, and referred to the case at the time.

CASE V. On the morning of 13th November 1899, Dr W. Carmichael telephoned to ask me to see an old gentleman with him who had symptoms of intestinal obstruction from what

seemed to be intussusception, showing itself at the anus. The patient was eighty-three or eighty-four years of age, although with the vigour and appearance of a man fifteen or twenty years younger. He was in great distress, not only from the obstruction of the rectum, but also from retention of urine due to an attack of congestion in an enlarged prostate.

He said that as far back as 1893 he had been troubled with a discharge of mucus and blood from the rectum. Bismuth had been prescribed for him at that time, and under its use the discharge had ceased. From time to time since then the discharge had reappeared, and had disappeared under the use of the same remedy. During the last six months his bowels had been much constipated. He required to take aperients frequently, and after taking one he had always severe diarrhœa and a feeling that the discharge from the bowel was never complete. Having failed to get any action of his bowels that morning, he had sent for Dr Carmichael.

His bladder was distended, and he was distressed with almost constant tenesmus and spasms of pain. Examination with the finger showed the rectum to be occupied by an unmistakable intussusception. During a spasm it reached nearly to the anus. From the history I thought it probable that this intussusception had been caused by some new growth partially obstructing the lumen of the bowel, and the probability was that it might not be very malignant, as it had taken so long to grow. The urine was drawn off, and efforts were made to reduce the intussusception by enemata. In the evening I saw him again, and as the condition of the bowel was not improved I operated at once, *i.e.* about twelve or fourteen hours after the onset. Acting on the supposition that the seat of mischief was in the lower part of the great intestine, I opened the abdomen in the left inguinal region. The upper part of the sigmoid flexure was drawn tightly down upon the brim of the pelvis, and felt like a firm thick cord as it passed into the intussusception. The rest and greater part of the sigmoid flexure was, of course, invaginated into the rectum and firmly grasped by it. Fortunately there had been no time for adhesions to form, and after considerable difficulty, I was able to undo the intussusception. When withdrawn the sigmoid flexure was found unduly long, and it contained about the middle

a firm mass, evidently of new growth, the cause, as we had suspected, of all the trouble. As the patient's strength at this stage of the operation was good, and as the tumour seemed to be quite limited, I decided to excise it and suture the ends of the bowel. Clamping forceps were applied about two inches distance from each side of the tumour, and a large Paul's tube was tied into the part above the tumour. After relaxing the upper clamp, the loaded colon was relieved of a large quantity of gas and fæces through the tube. The clamp was then replaced, and the bowel excised. The cut edges were united with a double row of continuous fine silk stitches. Some bicyanide gauze was packed into the neighbourhood of the sutured bowel, and the wound closed.

Next morning I found that he had vomited a good deal during the night and had been very restless, and on examining the wound, I was horrified to find that the small intestine was protruding through the wound. I remembered then that at the end of the operation I had trusted to the split abdominal muscles coming together, and had put in only a few deep catgut stitches, besides sewing up the peritoneal coat with a continuous suture. The pulse had been growing feeble, and it had seemed important to finish quickly. However, in the course of the operation I must have gradually enlarged the wound more than I had supposed, and the retching had burst it open. Fortunately my instruments were still in the house, so I washed the intestine with boiled water, returned it, and united the muscular layers of the abdominal wall with chromic catgut without an anæsthetic, and with very little inconvenience to the patient. No bad result followed from this accident, and it will serve as a warning to all of us to attend to details even at the end of a long operation at night, and when it is important to get the patient back to bed as soon as possible.

In the after-treatment, my chief anxiety was lest in defæcation the resistance of the anus might cause a strain to be put upon the recently sutured bowel which it could not withstand. The anal sphincter was in a relaxed condition, fortunately (possibly from the previous straining), so on the third morning, after a laxative the night before, I inserted a small Sim's vaginal speculum and douched out a quantity of fæces that lay in the rectum, and afterwards carefully washed out the

bowel higher up. This process was repeated every second day for about a fortnight. We had a great deal of trouble with the bladder, which was paralysed for some time from the over distention. Cystitis set in, but eventually this was overcome, and he has now (20th May, date of publishing lecture) recovered the use of his bladder, with only an ounce of residual urine, has a regular daily action of the bowels, and is in better health than he was for some time before the operation.

In considering these four cases of acute intestinal obstruction, and one which simulated that condition, there are many points to which attention should be paid, but I shall dwell especially on those which illustrate the importance of thorough investigation before operation. A study of the meaning of symptoms being important for this end, we must attend to a few of these too. One of the most important details in treatment must also be referred to.

The value of a careful consideration of the history was shown in several of the cases. Thus it led us to conclude correctly in the first case that some old-standing tuberculosis was present in the abdominal cavity, and that a partial obstruction had been made complete by errors of diet. In the last case it prepared us for finding a tumour of the bowel which might be suitable for excision. The want of a correct knowledge of previous drinking to excess was a disadvantage in the second case, and might have been more serious than it was. If anything could have saved that patient after purgatives failed, it would have been an emptying of the bowel by laparotomy, and a knowledge of previous drinking would have made us realise the early need of that step more than we did. In the fourth case the history of illness dating from child-birth was more important than we took it to be. The symptoms complained of did not point specially to pelvic mischief, but they probably arose from it.

One of the best known symptoms of acute obstruction is persistent vomiting. The little boy (Case I.) had a history of that, but did not show it after his admission. In Case III., although there was complete obstruction, the history was only of occasional vomiting. In the fifth case vomiting had not occurred, but this is not surprising when the great intestine was the part involved. We may state generally that the

higher in the intestine the obstruction the more rapid the onset of vomiting, and vice versa. Why the child ceased to vomit after his admission, and why there was only a comparatively small amount of vomiting described in the third case, we need not stop to discuss. At present it is sufficient to note the point in each case, as it might have been misleading if taken by itself.

A symptom of considerable importance is the outline of coils of distended bowel showing themselves on the surface of the abdomen. It was present in the first, third, and fourth cases. By Professor Wyllie and Mr Treves it is looked upon as very rare in acute cases, and if present in them at all as indicating that an acute or complete obstruction has supervened upon a chronic or partial one. It was this symptom, among other indications, that led me to take such a view of Case I., but Case III. showed the symptom also, and was an instance of obstruction becoming acute or complete at once. We only saw the patient in the later stages of his illness, but it is worth remembering that the symptom was undoubtedly present then. As an exception proves the rule in the sense of testing it, I shall in future be on the lookout for finding prominent coils present in obstruction acute from the first.

The pain in these cases varied considerably. Where the bowel was not injured by pressure, as in Case I. and Case IV. when admitted, the pain seemed only that caused by the spasmodic efforts of the bowel to overcome the obstruction; where the bowel was injured by constriction, and where peritonitis was also present, Case III., the pain was constant, with aggravations when the spasms came on.

A careful study of the character of the abdominal distention and its relation to the percussion note may show us that the bowel is distended with fluid. Taken along with symptoms of obstruction this may guide us to the obstructed point—for the bowel seems to be most loaded just above the obstruction—as in Cases I. and III.; or when there is a general distention of the bowel with a dull note, and no free fluid in the peritoneum, as in Case II., we will be able to grasp the importance of early evacuation of the bowel by purgatives, or failing that by operation.

You will have perhaps noticed that it was mentioned

in the account of Case III. that no vaginal or rectal examination was made. This unfortunately is one of the instances where we learn the importance of a step by finding out the disadvantage of not employing it. It did not seem worth while to make the examination. A clinique was going on when I was asked to see the patient; her symptoms did not seem to point to the pelvis, but to the abdomen as the seat of mischief, and yet the trouble really was in the pelvis all the time. Had a pelvic examination been made our view of the condition would have been different, and possibly an earlier operation might have been undertaken than the symptoms of obstruction seemed to warrant. All that seems very obvious now, but the lesson it teaches us, of taking nothing for granted which can be investigated, is a very difficult one to master. "Verify your references."

The only point in connection with treatment that I intend to discuss now is the emptying of the bowel above the obstruction. The object of this procedure usually is to get rid of gas and fermenting fluids, and this is most important. Lying in the intestine these accumulations are not only apt to poison the system by absorption into the blood, but may give rise to peritonitis by transudation through the intestinal walls. In Case I., however, an unexpected advantage resulted. There were lumps among the contents of the small intestine which would have blocked the narrowed bowel again, although the calibre of the narrowed part was still large enough for the purposes of digestion of wholesome food. The subsequent progress of that case showed this. Later on, when the boy was delirious and apparently becoming dangerously ill again, I gave him calomel, and from the time that he had a free action of the bowels, his convalescence began.

While we have tried to study the meaning of individual symptoms, it will perhaps have been seen that it is not safe to trust to one or two. Our object should rather be to weigh out all the facts of the case, and to let the diagnosis be built upon them, balancing one thing against another.

A deduction which may be drawn equally from the unsuccessful and from the successful cases, is that early operation gives the best chance of recovery. Case I. taken at the end of three days was nearly too late; Case III. was so at the end of

four days, although the operation was justified as giving the only chance of life. Case VI. was a peculiarly difficult one, but an earlier operation might have been successful. Case V. taken about twelve hours after obstruction began stood well the somewhat prolonged operation necessary to give a permanent cure.

In conclusion we may sum up what has been said on the subject of acute intestinal obstruction in two short sentences—"Knowledge is power," "When in doubt operate."

VAPORISATION (ATMOKAUSIS) OF THE ENDOMETRIUM

Read before the Edinburgh Obstetrical Society

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IN the *Centralblatt für Gynækologie* for January 19th, 1895, Dr Neugebauer of Warsaw gave preliminary notice of a Russian paper by Professor Sneguireff of Moscow, who had for some years previously been making observations on the effect of steam as a hæmostatic. It had proved serviceable in various hæmorrhages, but the application that to a gynecologist was specially interesting was the use of it for arresting uterine hæmorrhage and modifying the character of the uterine mucosa. It was stated that the vaporisation of the endometrium has an action at once caustic, hæmostatic, anæsthetic and antiseptic. The remembrance of this notice was among the elements that made me desirous of visiting Moscow on the occasion of the XII. International Medical Congress in 1897. There I had the satisfaction of hearing Professor Sneguireff's exposition of his invention; and impressed with the value of it, I was about to procure the apparatus, when the Professor generously offered to send it after me to Edinburgh. It consists of a kettle for generating the steam, and a double catheter for conveying the hot vapour into and out of the uterine cavity. The central tube carries in the steam, which applies itself to the endometrium and escapes through the outer tube that has a series of fenestrations in its intra-uterine extremity.

I shall refer presently to the cases in which I have made

use of this instrument. Meantime let me indicate the important service Dr Ludwig Pincus of Danzig has rendered to the profession in popularising the procedure, testing its applicability in a variety of cases, studying the conditions under which it may be employed with most safety and certainty, and modifying the apparatus in some more or less important directions. He applies to the operation the designation Atmokausis (ἀτμός = steam). The instrument he calls the Atmokauter. These terms are being widely accepted. In some cases he uses a non-fenestrated catheter in which the steam heat is applied to the interior of the uterus through the wall of the outer tube, the steam not being allowed to escape into the cavity of the uterus. This steam-hot catheter he calls the Zestokauter (ἡ ζεστότης = boiling heat), and the operation Zestokausis. He has written various papers in the *Centralblatt für Gynækologie* and elsewhere, and the Transactions of some Societies and Congresses contain records of his enthusiasm and industry. Two lectures by him in Volkmann's *Sammlung Klinischer Vorträge*, Nos. 238 and 261-2, for March and December 1897, respectively, contain the fullest exposition of his experiences and furnish very complete reference to the literature of the subject. It will give the clearest idea of what may be expected from the employment of Sneguireff's Vaporiser in gynecological practice if I quote the propositions which Pincus has formulated in the last of these addresses which was delivered before the Obstetrico-Gynecological section of the 71st Versammlung Deutscher Naturforscher und Aertzte, München, in September 1899.

"1. Atmokausis and also Zestokausis are both effective and serviceable therapeutic methods. They are not more dangerous than other intra-uterine procedures if the technique is fairly understood, and the prescribed rules carefully followed. Any possible dangers stand in no relation to the good it effects. No one can decline the responsibility of testing and appreciating the methods. The question of their necessity is to be answered affirmatively.

"2. The technique of the procedure may be considered as settled. It regards in the first and only place the demands and needs of practice. The method has been developed to a typical method of cure.

"3. The best results are obtained when the method is applied in a typical manner, *i.e.* without narcosis; apart from curetting; with dilatation and protection of the cervix; strict individualisation; rest in bed till separation of the slough—in slight cases from the third to the fourth day.

"4. A dosage of the cauterisation from a superficial effect to the most complete destruction of the mucous membrane is throughout absolutely possible. The dosage depends on the height of the temperature (steam tension), and the consistence (contractility) of the uterus. The higher temperatures from 110-115° C. are to be preferred, because in that case the influence even of the shorter application is the same. The principle holds good—shortest possible duration, highest possible temperature (maximum 115° C.). Especially in young women and again especially after abortion one ought to employ the shortest duration. Rather repeat the application, if necessary, after regeneration of the mucosa (about four weeks), or wait recurrence of next menstruation. This precaution is necessary till one attains certainty in judging of a concrete case, *i.e.* till one is quite accustomed to the use of the method.

"5. Both procedures may replace or supplement curettage. They are quite indispensable when the curette fails.

"6. The method presents a specific against uterine hæmorrhages from whatever cause (menorrhagia, metrorrhagia, climax, myoma), and produces good and rapid results in all forms of endometritis, metritis and subinvolution.

"7. Obliteration ought only to be sought for to prevent the necessity of total extirpation, never for prevention of cancer of the uterus or from other indications. In any case the indicatio vitalis should be given, for cases are known in which the consequences of obliteration have necessitated total extirpation and castration.

"8. Where the atmokausis fails or produces only transitory effect, the contractile elements of that uterus have been quite lost.

"9. Atmokausis in inoperable intra-uterine cancer is the best palliative yet known. It has also proved serviceable for disinfection of the cavity of the uterus immediately before total extirpation and abdominal operations.

"10. In puerperal sepsis (abortion and full time) it produces excellent results in the early stage. In severe general infection it is useless.

"11. Submucous myomata (polypi) and malignant neoplasms, when these have been proved by microscopical examination or by the touch, must be excluded from this treatment.

"12. A condition *sine quâ non* for the application is absolute freedom of the adnexa. Whether it is applicable in gonorrheic and other acute processes of the uterus is uncertain."

Dr Barclay, Buchanan Scholar, has looked up the records of fourteen cases in which we have used Sneguireff's Vaporiser in the treatment of uterine disease in the Buchanan Ward of the Royal Infirmary. They were all cases of more or less pronounced endometritis with menorrhagia or metrorrhagia in women varying in age from thirty to forty-five. The vaporisation was always carried out under chloroform narcosis, because the instrument was used in the presence of members of the clinical class, and the application was continued in some for forty-five seconds, in others for sixty, and in one for ninety seconds. One patient alone complained of any suffering. It had been applied for sixty seconds, and it is noted that for some hours after she had uterine pain, which, however, passed off, and she was dismissed cured. The records bear that of the fourteen cases eleven were cured and the remaining three were improved.

My experience, accordingly, does not permit me to accept offhand all the statements of Dr Pincus; but in the main I must regard them as well grounded. I agree with him, *e.g.* in suspecting misapplications of the procedure in cases of misadventure. The case of v. Guérard (*Centralblatt für Gynækologie*, xxiii. 1081, Sept. 1899) can hardly stand to the discredit of vaporisation. Subjected at the age of thirty-one to ventrofixation of the uterus for sacral pain, headache and general discomfort, the patient aborted in January 1890 at the fifth month. The labour was tedious and difficult, and followed by long weakening hæmorrhages, for which atmokausis was applied in April. This was followed by amenorrhœa and such insufferable pains at what corresponded to menstrual epochs, that an attempt was made at vaginal hysterectomy. The strong, very vascular, adhesions of uterus to abdominal

wall made it needful to attack it by a laparotomy, which allowed of removal of uterus and adnexa only with the greatest difficulty. The adnexa on each side had been converted into inflammatory tumours the size of a small child's fist. From such a case no conclusion can be drawn as to the value of any kind of operative intervention. It shows, however, that such a destructive effect may be produced as to lead to complete atrophy of the endometrium—an effect which is sometimes desirable and which can with care easily be avoided in treating women under forty years of age who have not yet finished their reproductive career. One of the most interesting results I met with was in the case of a ward-maid in the Royal Infirmary, in whom two years previously diseased ovaries had been removed. She had for a time been amenorrhœic, but irregular metrorrhagic discharges occurred which led us to consider the necessity of vaginal hysterectomy. It was decided, however, first to make trial of the atmokauter, with the result that the hæmorrhages ceased and atrophy of the uterus set in.

Another drawback that has been charged to the procedure is the possible production of a stenosis or atresia of the cervix. Against this contingency Pincus provides by having the catheters covered with celluvert. I have found no difficulty in using the original Sneguireff instrument covered according to Dührssen's suggestion (*Centralblatt für Gynækologie*, 18th March 1899), with a piece of india-rubber tubing.

The germicidal power claimed for the steaming of the uterus must be doubted, if not discarded, in view of the experiments of Flatau (*Monatsschrift für Geburtshülfe*, x. 337, 1899), who finds that it takes a minute before the temperature rises to 75-80° C., and that the available heat even after the sixty seconds is only from 75-85° C. Flatau, however, recognises the value of the procedure, especially in cases of preclimacteric hæmorrhages and of interstitial endometritis.

All are agreed that it is important to take note of the condition of the uterine walls, the best results being obtained where these are firm with good power of muscular contraction; and the only really disastrous accident that has been recorded having occurred in Treub's Klinik in Amsterdam (*Centralblatt für Gynækologie*, No. 52, 1898), in a case of soft-walled uterus

where the fundus was perforated by means of the zesto-kauter.

It will have been observed that Pincus lays stress on the application of the vapour without bringing the patient under the influence of an anæsthetic, chiefly on the ground that when the steam has produced its effect on the mucosa the muscular walls begin to be irritated and painful contractions usually arise which give a signal that the current should be turned off. He not only applies the procedure to non-narcotised patients, but considers it safe to use it in ambulants. He recommends, however, that when a patient has been atmokauterised at a poliklinik or a consulting-room, she should be made to go to bed and rest for three or four days or until the slough separates. In all the cases in which I have employed the instrument, the patients, as I have stated, were anæsthetised and kept at rest for two or three weeks. There has been no untoward effect, even though in some of the cases there were traces of old-standing salpingo-ovaritis.

In America, Dr Brothers of New York (*American Journal of Obstetrics*, xl. 498, 1899) has published his encouraging experiences with intra-uterine vaporisation, but I have not observed that British journals have as yet shown an interest in the subject. It is, however, well worthy of more study than has been accorded to it, and I am convinced that wider experience will only go to confirm the impression that this intra-uterine vaporisation or atmokausis will retain a permanent place among the curative measures at the disposal of the gynecologists.

It should be added that Professor Sneguireff has also employed steam for the arrest of some forms of surgical hæmorrhage, as, *e.g.* in operating on the liver, with satisfactory results. He has shown that bleeding wounds that were steamed heal by the first intention, and his apparatus contains a tube for application of steam to wound surfaces in general. Of such atmokausis, however, I have no experience.

CLINICAL NOTE ON A CASE OF VULVAR HÆMATOMA[1]

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CASES of vulvar hæmatoma are sufficiently uncommon and sufficiently diversified to warrant recording in the Transactions of our Society. The case to be described occurred in connection with the midwifery department of the Western Dispensary, Edinburgh.

On November 29, 1899, at half-past five P.M., a message reached me from one of the Western Dispensary pupils, stating that a swelling had suddenly developed in the left labium of a parturient patient whom he was attending. The message had been sent off about 5.15, and, as I found out later, the vulvar swelling had then only just begun to form. I was in attendance at 5.45, and the tumour had then attained a size nearly equal to that of a fœtal head; so that in the space of not more than forty-five or fifty minutes the hæmatoma (for that was evidently its nature) had grown to very considerable dimensions. The patient was a primipara, aged twenty years and nine months; she was a domestic servant, but having become illegitimately pregnant she had returned to her mother's house, where she was now being confined. As I afterwards discovered, her pregnancy had progressed normally and without incident, and she had not shown any degree of pendulous abdomen (a condition to which Halliday Croom[2] is inclined to attribute etiological importance in connection with the vaginal variety of hæmatoma). She calculated that she was at the full term of gestation. Labour had commenced in the morning of the 29th, and the first stage had lasted about nine hours. The membranes had ruptured a short time before I was sent for. The student in attendance had noticed no special varicose veins in the labia, and the first stage of labour had been in no way abnormal. With the beginning of the second stage, however, the patient began to complain of sharp pain in the left

[1] Read before the Edinburgh Obstetrical Society, Wednesday, May 9, 1900.

[2] Croom, J. Halliday, *Trans. Edinb. Obst. Soc.* xi. p. 80, 1886.

side of the genitals; and about a few minutes after five o'clock, the swelling in the left labium majus had commenced to show itself.

I found on my arrival that the student in attendance, becoming alarmed by the rapid growth of the labial swelling, had sent out in haste for a neighbouring medical practitioner (Dr Richardson). He was present and kindly gave me his help and advice during the time occupied in delivering the woman. A glance at the patient showed that she had lost much blood, and it was at the same time noticed that there was some œdema below the eyes; she was complaining greatly of pain in the left labium majus. Inspection of the external genitals revealed a tense swelling in the position of the left labium, bluish black in colour, of the size of the closed fist, and extending for a short distance into the vaginal canal. The skin covering it seemed distended almost to bursting, and palpation did not detect any pulsation in the mass, which was partly solid and partly fluid; there was an obscure sense of fluctuation at one part. The vaginal examination discovered the fœtal head engaged in the pelvic inlet, lying in the O.L.A. position, well flexed; no contraction of the brim could be detected. The diagnosis could not be in doubt—the condition was that of vulvar hæmatoma or thrombus developing during the second stage of labour—the treatment, however, was not so evident. Since the hæmatoma was rapidly increasing in size (it was literally visibly growing), it was clear that there was no time to be lost; and the case resolved itself into the choice of operative interference, for it was evidently out of the question to leave it alone. As the blood tumour was actively increasing I hesitated to make an incision into it. On the other hand, I doubted whether I could apply forceps to the head without rupturing the hæmatoma. I decided to try forceps application. With considerable difficulty I got the blades in position, and easily brought the head on to the perineum. At the moment when I was making the head distend and pass over the perineum, the tumour bulged into the fenestra of one of the blades and ruptured almost with a clean cut tear. A huge mass of blood clot was expelled along with some fluid blood and serum.

I quickly thereafter delivered the head and body of the infant, which was a large male in a state of apnœa; it was soon resuscitated and is living at the present time (age, five months). It was not an abnormally large infant, neither was its head preternaturally well ossified. The placenta and membranes were removed manually from the vagina, and by this time the hæmorrhage from the hæmatoma had almost entirely ceased. Notwithstanding the escape of the large clot, the left labium was still somewhat larger than normal, the tissues seeming to be infiltrated with blood. The parts were carefully washed with weak perchloride of mercury solution, and I attempted to bring together the edges of the tear by means of silk-worm gut sutures (four or five in number). On account of the thickened condition of the labium, the wound could not be entirely closed; it gaped at the middle, so I had to content myself with loosely packing in iodoform gauze as a drain there. Some strips of gauze were also placed loosely in the vaginal canal, not with the purpose of plugging it, but simply to prevent if possible the infection of the wound. Iodoform powder was dusted copiously over the surface of the labium. The patient naturally enough was somewhat collapsed. She made, however, a perfectly satisfactory recovery: her temperature never rose above 100° F.; the wound was dressed twice daily; and in three weeks she was up and about. Now, five months after the confinement, the labium majus has returned to its normal size and there is nothing to indicate that it was ever the seat of a large hæmatoma. It may be added that a trace of albumen was found in the urine during the first few days of the puerperium; this disappeared under strict milk diet, as did the œdema under the eyes referred to above.

Remarks.—The case just narrated forms a very characteristic instance of one variety of thrombus or hæmatoma complicating labour; that, namely, in which the blood escapes into the tissues of one or other or both labia and to some extent into the vaginal wall also. It is rarer to find the thrombus entirely in the vagina, and rarer still to meet with the intra-pelvic variety; a most exceptional form is the pedunculated and superficial vaginal hæmatoma. It occurred during the second stage of labour, but statistics show that a thrombus

most commonly forms during the puerperium, less frequently during the second stage of labour, and very rarely during either the first stage or in pregnancy. It showed very clearly the three leading symptoms, pain, swelling, and hæmorrhage: the pain was sharp and was situated in the position occupied by the tumour, which was large, but not so large as has been reported on some few occasions; and the hæmorrhage was first of all interstitial into the substance of the left labium majus, and later external on account of rupture of the skin covering the labium. The rapidity of formation of the blood tumour was very noteworthy, and this is a commonly observed character of thrombus of the vulva. Fortunately the symptomatology did not include infection of the sac after rupture, and septicæmia was avoided. The diagnosis was easy, and it is only in the vaginal and intra-pelvic varieties that any difficulty in this matter is likely to occur. The treatment was not entirely satisfactory, and doubtless I would have been well advised had I incised the thrombus either before or during the application and employment of the forceps, for then I should have had a clean incision to bring together with sutures, and I could have made it at the point of election for drainage. At the same time I had hopes that I might have drawn the head over the perineum without rupturing the hæmatoma. After rupture had occurred, evidently the treatment resolved itself into prevention of infection of the cavity in the labium by the lochia, and fortunately this was successfully accomplished.

The *etiology* of the hæmatoma in this instance, as indeed in most instances, is most obscure. Most of the commonly alleged causes, such as traumatism, twin-labour, large fœtus, excessive ossification of the fœtal head, and unusual varicosity of the labial and vaginal veins, were conspicuous by their absence. There was, further, no reason to suspect the presence of a hæmophilic tendency in the patient, and the pelvic brim was very slightly, if at all, contracted. It has already been noted that there was no pendulous abdomen during pregnancy. Doubtless there existed the usual congestive tendency in the pelvic tissues, and the same changes in the maternal blood which are constantly met with in pregnant women, and which have by some been regarded

as causes of vulvar hæmatoma in labour and the puerperium; but it is not reasonable to ascribe an etiological importance to these *constant* conditions in attempting to explain the origin of so *rare* a morbid state as vulvar thrombus. The most that can be said is, that under some quite exceptional combination of circumstances the congestion of pregnancy and the accompanying hydræmia may act as causes predisposing to hæmorrhage. In the case just narrated it is possible that one of the combined circumstances that determined the effusion of blood was the albuminuria, for it will be remembered that after labour was over a small quantity of albumen was found in the urine (drawn off by catheter), and there was also the transient œdema of the face. It is evident that in searching for the causes of a complication of labour and the puerperium, which occurs but once in from 1500 to 1600 confinements, it is necessary to look for either one very exceptional circumstance or for a very unusual combination of less exceptional circumstances. Neither primiparity nor the pressure of the fœtal head on the congested maternal passages can be regarded as exceptional circumstances. All that it seems safe to state regarding the etiology of the case which I have described is that the strain of labour no doubt determined the hæmatoma which had been predisposed to possibly by the transient nephritis. It is interesting in this connection to note that in a recently reported case of hæmatoma vulvæ, occurring some hours after labour in a ii-para, H. Löhlein[1] discovered that there had been chronic nephritis in the pregnancy, and was inclined to attribute the hæmorrhage to that disease (*causa disponens*).

The pathogenesis of the hæmorrhage into the labia is no less obscure than the etiology. In the case just narrated no explanation can be hazarded, for the patient recovered, and there was happily no occasion for a necropsy. It has, however, been found (in a specimen examined by Perret[2]) that a sliding or displacement of the layers of tissues in the pelvis, and more especially in the vaginal walls, has taken place, and that into the space thus produced the blood has been poured. In one case the hæmatoma was found on one side of the

[1] Löhlein, H., "Zur Entstehung und Behandlung des Hæmatoma vulvæ der Neuentbundenen," *Centralbl. f. Gynäk.*, xxi. 267, 1897.

[2] Tarnier, S. et Budin, P., *Traité des accouchements*, iii. 667, 1898.

pelvis, while on the opposite side was a cavity capable of holding a hen's egg, but empty; it was supposed that this cavity was destined to be the receptacle of the outpoured blood, and that in fact the space into which the blood was to be effused was formed before the hæmorrhage took place. With the soft parts *in situ*, it must be remembered that the pelvic canal is wide above and narrow below, and that as the fœtal head is pressed downwards into it, it is not unlikely that the soft parts may be displaced downwards in front of it, thus producing a separation of the strata or layers which make up the vaginal walls and their immediate surroundings; into the space thus produced, blood may be poured either in labour (producing hæmatoma in the second stage) or soon afterwards in the puerperium (Budin).

HEREDITY IN DISEASE

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THE human body, like that of all the higher plants and animals, is a cell-community. The cells are mostly adherent, and all have descended from a common ancestor, the fertilised ovum; but in a very real sense every cell is a distinct and separate living entity, a unicellular animal. Blood-cells are free, germ-cells are purely parasitic, skin-cells have often been transplanted; there is little doubt that had we the requisite skill it would be possible to transplant every other kind of cell. No single cell in the body is the offspring of any other co-existing cell or group of cells, but every cell is derived from a pre-existing cell. *Omnis cellula e cellulâ.* The whole cell-community is separable into two distinct but very unequal parts: into germ-cells, and systemic (somatic) cells. To the former belong the function of continuing the race, to the latter the function of protecting the all important germ-cells.

A few years ago it was universally believed that acquired characters were transmissible. It was thought that changes in body and mind—caused by exercise, disease, accident, or what not—affected the germ-cells in such a special manner, that the traits the parent acquired tended to reappear as inborn characters

in the child. At the present day the majority of those who have given real scientific attention to the question believe the contrary. The problem is of obvious importance to medical men, who, however, as a body have curiously neglected it. Before beginning its discussion it is necessary to define a couple of terms, the loose use of which has resulted in endless confusion.

An *inborn* character may be defined as one which results in the individual from the constitution of the germ-cell (or pair of germ-cells) whence he sprang. Thus a man's ears, eyes, nose, etc., are inborn characters. They arise because his germ is so constituted that under fit conditions of shelter, nutrition, etc., it tends to proliferate into an organism having those characters. An *acquired* character, on the other hand, is one which results from the action of the environment on the soma—the systemic or body cells as distinguished from the germs. Hence all characters produced in the individual by modes of life, by exercise, disease or accident, are acquirements. To take an example, suppose the child of a normal man is blind; then the blindness is inborn if due to a defect in the germ whence the child sprang, but acquired if due to disease or accident to his visual structures *after* they have developed from the germ. The error is often made of supposing that all new characters are acquirements. Thus a supernumerary digit on its first appearance in a family is often spoken of as one. It must, however, be clearly borne in mind that the distinction between the inborn and the acquired is not one between the old and the new; it is wholly one of origin. Inborn characters originate in the germ-cell; acquired characters in its descendants, the somatic cells. As a fact, every character acquired or inborn is new to every individual who has it; thus a man's head is as much a novelty to him as a supernumerary digit. Every child differs somewhat from its parent. When a difference is inborn (*i.e.* when it is due to a germinal peculiarity), it is technically termed a "variation." All acquirements, on the other hand, are termed "modifications." It should be noted that a modification (unlike a variation) does not necessarily imply a difference from the parent; both parent and child may acquire similar modifications. The cause of variation is yet a matter of dispute. Many elaborate theories have been enunciated, which, at the least, afford splendid testimony to the imaginations of their authors. We need not dwell

on them ; suffice it to say that variations do occur, no matter how produced. Men do differ in inborn characters from their parents.

The body, as I say, is a cell-community, composed of germs and other cells. Now suppose a man acquires a character ; suppose, for instance, he strengthens his arms by exercise, or weakens his legs by laziness, then, if his acquired character is to be transmitted, the change in his arms or legs must so affect his germs, that it will be reproduced (as an inborn character) in the children into which the germs proliferate. This consideration at once reveals the difficulty of all belief in the transmission of acquired characters. How can each one of the million changes which may occur in the arms, legs, and other parts of the body affect the germs (situated it may be far distant) in such a special manner that after fertilisation and long separation from the parent organism the germs will proliferate into beings that have inborn the *particular* character the parent acquired ? How, for example, can a modification of the parent's great toe affect his germ differently from a modification in his little finger ? What is the machinery by which this magical process is carried out ? *A priori*, therefore, the transmission of acquirements seems impossible ; or at least so highly improbable that only the clearest evidence should satisfy us. Is such evidence forthcoming ? It is a simple fact that though the whole plant and animal world has been ransacked, no single instance of the transmission of a single acquired character has yet been proved. It is easy to say that important changes, such as those caused by constant hard exercise or continual laziness, must affect the germs, and through them the offspring. All that is not denied. It is the *particular* effects which are denied. It is not denied, for example, that the exercise which strengthens the arms, or the laziness which injures the legs, *may*, through changes in the nutrition, in some way or other, for good or evil, affect the germs ; it is only denied that the arms or legs of the child will be affected more especially than the lungs or the liver. Indeed we may go even further and say that, while it is theoretically possible for acquired characters to affect the germ, this is not *known* to be true ; for instance it is not known what effect, *if any*, exercise or laziness has on the germs. All that can be postulated with certainty is that acquired characters do not so

affect the germ that the child inherits the precise peculiarity the parent acquired, or anything like it.

The modern denial of the transmission of acquirements is really founded on, or should be founded on, the cell theory. Before its enunciation the different parts of the offspring were supposed to be derived from similar parts of the parent. Man, for example, was regarded as an individual. We know now that from the standpoint of heredity he is a community—a community of specialised cells—of which one set, the germs, are specialised for the production of similar cell-communities, just as other sets are specialised for the production of motion or of bile. We have therefore no more reason to suppose that muscle or liver cells play a part in heredity than to suppose that the germ-cells play a part in movement or in the production of bile. Formerly, when the transmission of acquirements was believed without question, some remarkable hypotheses to account for the supposed transmission were formulated, occasionally by very distinguished men. Darwin's theory of Pangenesis and Spencer's theory of Physiological Units are examples. These speculations were what are known as "working hypotheses"—hypotheses, that is, in which the amount of theory, perhaps I should say guess work, is wholly disproportionate to the foundation of fact, and which a more extended acquaintance with the facts usually reveals to be misleading. At the present day, when the transmission of acquirements is generally disbelieved, some working hypotheses, as remarkable, but running to the other extreme, have been formulated. Under very high powers of the microscope the nuclear matter of the fertilised germ may be seen to divide, apparently with great accuracy. On this slender foundation Weissmann has built his hypothesis of the continuity of the germ-plasm, adding amazing complications in the way of ids and idants and biophors, hypothetical bearers of heredity. Cheerfully entering into the regions of the unknown and probably the unknowable, he has attempted to explain *how* inborn characters are transmitted. Biologists in larger part have followed him.

His speculations, darkening counsel, crop up in every discussion on the subject. As a fact they have no essential bearing on it. The doctrine, first formulated by Galton, that

acquired characters are not transmissible is one thing, and is supported by abundant evidence. An attempt, on obscure intracellular grounds, to explain how inborn characters are transmissible, even if it involves the corollary that acquired traits are not heritable, is quite another thing. If successful it would afford additional proof of the former doctrine; its failure does not involve disproof of it. Galton's theory rests on the plain facts that each individual is derived solely from a single cell, the germ; and that there is no evidence that the somatic cells influence the germs in such a special and unlikely manner as to cause the *particular* characters the parent acquired to be reproduced by the children. It does not rest on more or less metaphysical speculations concerning the continuity of the germ-plasm or of ids, idants, biophors and so forth. It is to be deeply regretted that this red-herring of Weissmannism has been drawn across the trail. Weissmann's speculations, obscure in themselves, made more obscure by his methods of demonstration, still more obscure by being constantly emended under the influence of destructive criticism, have drawn many students from the true path, and by their difficulty have discouraged others. The actual issues involved are very simple. Either the germ-cells are influenced by the other cells in particular directions—in millions of particular directions—or they are not. Not a tittle of evidence has been adduced to prove that they are. In other words, no single instance of the transmissions of an acquired character has been recorded.

The cell theory supplies us with cogent reasons for believing that acquired characters *cannot* be transmitted. To set forth *in extenso* the proofs that they *are not* transmitted would exceed the limits of this article, in which I have still to deal with the large subject of heredity in disease. One or two arguments I may use, however, partly because they have not to my knowledge been developed by other writers, and partly because they seem to me well-nigh decisive. The extra development of the blacksmith's arm is rightly called an acquired trait, since it arises from exercise, from use, not from germinal conditions. But no infant's arm develops into an ordinary adult arm without stimulation similar in kind as that which develops the blacksmith's arm, though less in degree. Therefore almost all that separates the infantile from

the ordinary arm is acquired. The same is true of most of the other structures of the body, which do not develop except under the stimulation of use. Thus the brain, heart, lungs, all develop in this manner. Man's physical acquirements are therefore vast. When are any of them transmitted? Every infant has to make afresh under similar stimulation the modifications its parent so laboriously acquired. If it be argued that exercise and use increase, not only the individual's acquirements, but also his power of making them, and that it is the latter which is transmitted, I have only to reply that here is a manifest error. In the passage from infancy to old age the power of making acquirements constantly declines. In the infant it is at a maximum; hence his development into adult man. In the old man it is at a minimum; it is almost lost. Where nothing is acquired nothing can be transmitted. The same arguments apply to mind. At birth the infant's mind is a blank. His subsequent mental acquirements are immense. Every single thing contained within the memory of adult man, every single word of a language, for instance, is an acquirement. But when are the contents of a parent's mind transmitted to the child? Again a man is capable of becoming a parent at any time between extreme youth and extreme old age; a woman from the age of thirteen or fourteen till nearly fifty. Between the birth of the first child and the last such an individual changes vastly. Under the stress and tear of circumstances, under the slings and arrows of outrageous fortune, all sorts of acquirements are made. The body becomes vigorous, and then feeble; the mind grows mature, and then senile. He or she grows wrinkled and bowed, and perhaps very wise, or perhaps much the reverse. Yet no one viewing a baby show, a children's party, or an assembly of adults, of whom he has no previous knowledge, can say which is the child of the youthful and which of aged parents. Apparently, therefore, the whole of the parents' acquirements have no effect on the child. Surely no arguments could be stronger.

It is true that some medical men maintain that the children of old men who have suffered from gout have a greater tendency to that disease than the children of younger men; but this is just one of those wild unconfirmed guesses

with which medical literature unfortunately abounds. No statistical proof has been advanced. *Post hoc* has been confused with *propter hoc.* Men with a gouty diathesis, an inborn trait, tend under fit conditions to develop gout; inborn characters are transmissible; children are usually placed under conditions much the same as their parents, therefore gouty men often have gouty children. There is nothing to prove that the parental acquirement, the disease as distinguished from the diathesis, in any way affects the offspring. The poor Irish peasantry do not suffer from gout. When they have the diathesis it remains latent. Removed to richer food and easier conditions in English cities, they suffer on the average as much as other people. There is nothing in fact but guess work, mere opinions unsubstantiated by close and accurate observation, to show that the younger children of a gouty family are more liable to gout than the elder. Mutilations are sometimes instanced by medical men as affording examples of the transmissions of acquirements. Seeing, as they daily do, injuries caused by accident, disease, and the surgeon's knife, medical men should be peculiarly qualified to judge. In a million instances there is no apparent transmission. In the millionth and one a man with an amputated leg has perhaps a son with a deformed toe; thereupon the transmission of an acquirement is triumphantly proclaimed. There is, however, such a thing as mere coincidence. It is forgotten that though every mother loses her hymen and nearly every terrier his tail, yet congenital absence of hymen and tail is still so rare as to be regarded with extreme suspicion. Maternal impressions are much relied on. A mother sees a mutilated or malformed man, and it happens her child is malformed. Here again the element of coincidence is forgotten; every woman sees malformations during her pregnancy, but very few have malformed children. The children of nurses and lady doctors are not peculiarly liable to malformations. Moreover the mother gets a mental impression; the child something quite different, a physical malformation; therefore in any case there is no transmission of the mother's acquirement. Often, as in the case of gout, diathesis is confused with disease. Thus father and son both may have an inborn (and transmissible) incapacity to resist the bacillus of tuberculosis. They

both get infected. Thereupon the son is supposed to have inherited consumption from the father. He merely inherits the incapacity to resist the bacillus. The acquirements, infection and disease, come later. Like the parent he acquires them for himself. Again, certain morbid conditions depending generally on structure are inborn, *e.g.* hæmophilia. The father, perhaps the first of his race to be so afflicted, transmits his inborn peculiarity, like other inborn traits, to the child. The new is then confused with the acquired, and the transmission of an acquirement is thought to be proved.

The debate on Heredity in Disease by the members of the Edinburgh Medico-Chirurgical Society, lately published in this journal, was very interesting to the writer. Impressed by the importance of the subject, he has long hammered at it without awakening much interest.[1] It is to be hoped Professor Hamilton will be more successful. It is in his favour that his audience, as proved in the subsequent debate, showed a grasp of the subject not very usual south of the Tweed, where the mention of heredity in medical circles provokes as a rule not argument but wrath. "Weissmann's locomachy" is denounced, and contempt is expressed for the "philosophic froth which has been poured about." "Abstract Philosophy," we are told, is not of importance in medicine, which it seems is a science of "tips"—Ipecac. is good for cough, Chlorodyne is good for stomach-ache, and so forth. To judge by appearances, the contempt in this case is not founded on familiarity.

A point of far-reaching importance, one which tells decisively in the controversy, was however left untouched by Professor Hamilton and those who followed him. If, as is alleged by most medical men, the effects of disease are transmissible, then these effects must accumulate generation after generation. The son must start with the parent's constitution plus the effects of the son's disease, and so on. It is plain, on this hypothesis, that a race afflicted by any disease should undergo evolution or degeneration—evolution if the disease tends to strengthen the individual against subsequent attacks as by conferring immunity, degeneration if it tends to weaken him. On the other hand, if the effects are *not* transmissible,

[1] Vide *The Present Evolution of Men* (London: Chapman & Hall).

then a race afflicted by deadly disease would equally change, would undergo evolution—but not degeneration. For men differ in their inborn power of resisting disease. Disease is a selective agency. It weeds out the less resistant to it, leaving the race to the more resistant. Therefore on this hypothesis a race afflicted by disease should grow more and more resistant to it. Moreover, since resisting power against one disease does not imply resisting against any other, for example, since resisting power against phthisis does not imply resisting power against malaria, every race should grow resistant only against the particular diseases by which it is afflicted.

To some extent, to a very slight extent compared with its importance, this subject has engaged the attention of medical men, and always it has been supposed that when races change under the influence of disease they do so through the transmission of acquirements. Thus Dr William Russell remarks, "That acquired characters can be propagated there is strong proof. It is generally accepted that the two commonest diseases in this country, namely, scarlet fever and measles, are much less virulent than they used to be, and this is almost certainly to be attributed, not to an attenuation of the virus, or to improved treatment, but to a measure of immunity acquired by a population whose progenitors for generations have passed through the ordeal of these infections. It is a historical fact that measles introduced into a virgin soil, unmodified by a degree of immunity acquired by transmission, proves a most virulent and fatal malady. Another historical instance is that of the company of Esquimaux who, taken to Berlin, were vaccinated to prevent them taking small-pox, and they nearly all died from the modified virus used without any risk for protective purposes by the ordinary European." [1]

Dr Russell's statement is certainly correct, but I think his deduction may be shown to be wrong. Moreover the extent and importance of the racial change mentioned by him is not often realised by medical men. All the races of mankind sprang from a common origin—Adam and Eve or a species of lower animal, it matters not which. But they have diverged vastly. There are big races and small, white and black, woolly and

[1] The *Scottish Medical and Surgical Journal*, April 1900, p. 330.

straight-haired, and so forth. Among other particulars, races differ greatly in their powers of resisting this or that disease. For example, Negroes are incomparably more resistant to malaria than Englishmen, who, on the other hand, are as highly resistant to tuberculosis when compared to Australian blacks. *It is most significant that every race is resistant to every deadly disease strictly in proportion to its past experience of it.* Englishmen and Polynesians, when emigrants in countries where malaria, typhoid, or dysentery are very rife, suffer much more than the native inhabitants. When small-pox, measles, tuberculosis, syphilis, scarlatina, plague, or yellow fever overpass their normal boundaries, and attack the inhabitants of countries where they were previously unknown, or little known, they are destructive to a very unusual degree, as in Polynesia. Dr Russell mentions an interesting example. Vaccinia is presumably a very attenuated form of variola. For centuries and until very recently we were scourged by small-pox; to the Esquimaux it was unknown. Most of us are able to recover even from small-pox; the Esquimaux perish even from vaccinia.

One attack of many diseases confers on the *individual* increased resisting power, or even complete immunity, against subsequent attacks of the same disease, as in the case of scarlatina and chicken-pox. There is a consensus of medical opinion that this increase of resisting power in the parent is transmitted to some extent to the child, and that thus, from its accumulation during generations, arises increased resisting powers in the race. But there are some diseases, of which tuberculosis is an example against which no resisting powers can be acquired. One attack of tuberculosis, a most death-dealing disease, weakens, rather than strengthens, against subsequent attacks. If, therefore, the acquired effects of disease are transmissible, races afflicted by tuberculosis should grow less and less resistant to it. The exact opposite is true. For very many centuries tuberculosis has ravaged the Old World, especially such crowded parts of it as England. But Englishmen now increase and multiply in cities and towns, the natural breeding places of tuberculosis, whereas, under like conditions, the inhabitants of the New World, where tuberculosis was unknown till recently, perish. When infected with

tuberculosis by white men they tend to extinction everywhere, even in rural districts. Plainly there has been great evolution, but as plainly it has resulted solely from the survival of the fittest, not in the least from the transmission of acquirements. On the other hand there are some diseases against which immunity may be acquired, but which, though prevalent, are not deadly. Chicken-pox is an example. Against them no evolution is observable, for chicken-pox is as severe in type when attacking Polynesians as when attacking Englishmen. In this case, in the absence of selection, the acquirement of immunity has not tended to render the race more resistant. It is clear, then, that races grow resistant, not through the transmission of acquirements, but solely by the survival of the fit.[1]

A study of alcohol and other narcotics is instructive in this connection. Indulgence in any of them—alcohol, for example, increases as a rule the desire for indulgence. The more a man drinks, the more within limits does he crave for drink. But the limits vary widely. In some men experience of drink quickly awakens a desire for deep indulgence. They have the alcoholic diathesis, an inborn trait. They become drunkards, unless withheld by moral or other deterrents. In other men experience of drink, no matter how prolonged, awakens nothing more than a desire for moderate indulgence. They have not the drink diathesis. They are temperate without effort all their lives. The reader probably belongs to the latter category; he is not tormented by a craving for drunken-

[1] It is to be noted that one attack of many diseases usually confers immunity on the individual, yet in such cases the race never attains to it. Each succeeding generation remains as susceptible to infection as the preceding. Thus Englishmen are as susceptible to infection by measles as Polynesians. But since measles weeds out those who cannot recover from it (*i.e.* those who cannot acquire immunity from it), the direction the evolution takes is towards an increase of the *power of acquiring immunity*. For that reason, though Englishmen are as susceptible to infection by measles as Polynesians, they recover from it much more easily. The only diseases against which inborn immunity is, or tends to be, evolved, are those against which the individual cannot acquire immunity—tuberculosis, for example. When immunity against disease can be acquired by the individual, then the power of acquiring it is evolved in the race by Natural Selection. When it cannot be acquired by the individual, when one attack weakens rather than strengthens, then inborn immunity is evolved in the race. In the one case the capacity to *recover from* infection is evolved; in the other the capacity to *resist* infection. In both cases the evolution proceeds wholly on lines of Natural Selection, not on lines of the transmission of acquirements.

ness ; he is therefore temperate, not because he resists temptation, but through lack of it. Yet even in his case indulgence has increased his desire for alcohol up to a certain point. Now this increased desire for drink, this increased capacity for enjoying it, this weakened capacity for resisting it, which springs from indulgence, is an acquired trait, and therefore, if acquired traits are transmissible, the race which has longest used drink should be the most inclined to drunkenness of all. The son would inherit the father's capacity for enjoying drink, plus the increment caused by the father's drinking ; the grandson would inherit the son's capacity, plus the increment caused by the son's drinking ; and so on. It is evident that, given a sufficient supply of alcohol, the desire would at length grow so intense and uncontrollable that the race would be exterminated. On the other hand, since men differ in the intensity with which they crave for drink ; since those who crave most, being exposed to the greater temptation, *as a rule* drink most ; since alcohol is a poison which poisons most those who drink most ; it must follow, if acquired characters are *not* transmitted, that this elimination of the unfittest and survival of the fittest will result at length in a lessened proneness to deep indulgence, to the elimination, in fact, of the drink diathesis, to the evolution of a more temperate race.

What are the facts ? Neglecting temporary fluctuations, generally of minor importance, caused by changes in fashions, or morals, or in the price of alcohol, etc., every race that has access to abundant supplies of alcohol, is temperate strictly in proportion to its past experience of the poison. There has, in fact, been no transmission of an acquirement, but merely a great survival of the fittest. The Jews and the inhabitants of the wine countries generally, who have been afflicted by alcohol for thousands of years, are the most temperate peoples on earth. North Europeans, who have had a less abundant supply, and have been less afflicted and for a shorter period, are much more drunken. Savages, who have never been able to manufacture alcohol, are so inclined to drunkenness, that in all parts of the world, from pole to pole, when given the opportunity, they drink to extinction and perish. They are as little capable of persistence in the presence of strong modern solutions of alcohol as they are of persistence in towns in the

presence of tuberculosis. It is the fashion to attribute national insobriety to all sorts of causes. To climate;—but savages in all climates are drunken, whereas Jews are sober. To strength of beverages;—but Wellington's English were drunken in the Peninsular when the native soldiers were sober; savages, if they have the opportunity, get drunk on the lightest beverages; the sober races of the present time, Jews, Greeks, South Germans and Frenchmen, Italians, Spaniards, and Portuguese, were anciently drunken when their wines were no stronger. To civilisation;—but North Europeans are more civilised but more drunken than South Europeans; West Africans, savages of a very low type who have long manufactured palm wine, are conspicuously more temperate than other savages. Parental or ancestral drunkenness;—but savages whose parents and ancestors never had any drink, are the most drunken of all. Education;—what is there in the education of South Europeans and Jews which renders them so superior in this particular to North Europeans? Temperament;—but how did differences in temperament arise if not through evolution? Advance what other hypothesis one will, and there are numerous exceptions; this theory of alcoholic evolution, which is in accordance with all we know of evolution under natural and artificial conditions, alone covers all the instances. Surely, then, there is no escape from the conclusion that all races that have had access to alcohol and are temperate have become so through evolution—through the elimination of the unfit, the innately drunken.

Opium has been used for hundreds of years in India, for two centuries in China, for three or four decades in Burma. The evidence given before the late Royal Commission on Opium showed that opium is never or very rarely taken to excess by the natives of India; it is frequently taken to excess in China; the Burmans take it in such excess and perish in such thousands of it that we, the English rulers of the country, are obliged to prohibit the use of opium in Burma to Burmans alone, while permitting it to all other nationalities; just as in Canada and Australia we are obliged to prohibit the use of alcohol to the natives alone, while permitting it to all other peoples.

Were acquirements transmissible, races that had longest used tobacco should crave most for it. As a fact, races to which it is newly introduced crave just as much for it as those who have used it longest. On the other hand, like chicken-pox—but unlike tuberculosis, malaria, alcohol, opium—tobacco is not death-dealing. As a consequence it is not a cause of evolution; races which have longest used it crave for it as much as those to which it has been newly introduced.

Biologists have sought far and wide for evidence for and against the transmission of acquired characters. All manner of uncanny experiments, generally difficult for the ordinary man to repeat, have been made on all kinds of strange animals, Guinea-pigs and white rats have been great sufferers. Had the experimenters turned to man, the proper study of mankind, their labours would have been needless. Had they considered the non-transmission of the effects of disease, surgical operations and chronic inebriety, a useless chapter in vivisection would not have been written. Had they considered the amplitude of man's physical and mental acquirements, they had scarce asked for proof more convincing.

Natural selection as a cause of evolution was alleged by Darwin. Thereupon his critics raised objections. First they declared there had been no evolution. They forgot how the races of mankind, though derived from a common origin, now differ each one from all others. Next they said there was no such thing as natural selection. Nature, according to them, worked in an altogether haphazard way. The fittest did not survive. This view found especial favour in the eyes of two aristocratic and orthodox amateurs, the late Duke of Argyle and the present Marquis of Salisbury—politicians whose cabinet rank gave weight to their opinions on matters biological. Some professional biologists agreed with or followed them. Thereupon experiments to prove or disprove the actuality of natural selection were set agoing. Professor Wheldon has lately done to death many crabs; Professor Poulton has caused the elimination of many chrysalises. But under the eyes of Cabinet Ministers and Professors alike, touching perhaps their nearest and dearest, worked those great causes of Natural Selection, those great agents of the elimination of the unfittest, disease and narcotics. They

sought for evidence of evolution! Drink and disease have produced evolution of such vast political and biological importance that the effects of all wars in all countries during all times fall into insignificance beside them.

Zymotic diseases may be divided into two classes which shade into each other. The first class comprises those diseases of which the pathogenic micro-organisms have their habitation entirely or principally in the human body; the second class, those of which the organisms inhabit principally the environment outside the human body, and to which a human prey is not necessary. Syphilis is an example of the first class; its micro-organisms inhabit wholly the human body, being communicated by one sufferer to another by direct contact only. They are therefore wholly parasitic, and parasitic on man alone. Malaria is an example of the second class; a human prey is not necessary to its microbes, which are abundant in many deserted or sparsely inhabited tracts, and are therefore largely saprophytic.[1] Between the two extremes, between the wholly parasitic and the mainly saprophytic diseases, are a number of other zymotic disorders which resemble syphilis on the one hand, or malaria on the other, with respect to the incapacity or capacity of their microbes to exist apart from the human body. The microbes of measles, tuberculosis, chicken-pox, scarlatina, small-pox, influenza, etc., are all earth or air borne. They are not communicated by direct contact, and can therefore exist at least for a limited time away from the human body; but appar-

[1] I use the word saprophitic, as meaning merely that the microbes are capable of persistence for an indefinite period outside the body of man. The life-history of the malaria parasite is not thoroughly known. It is certain that it infests the mosquito, and Dr Patrick Manson believes that it infects man only through the mosquito. Lately he has extended his theory, and now states that the mosquito is infected through man alone. "Malaria would be stamped out in a community by all round free drugging with quinine. After a month or two the supply of infected blood would have come to an end, and consequently there would be no more infected mosquitoes. That is one way of exterminating malaria." If Dr Manson be correct, the malaria protozoon is strictly parasitic. But there are difficulties. If man is infected solely through the mosquito, why does the digging of malarious soil frequently start an epidemic of malaria? It is not to be supposed that infected mosquitoes are dug up. On the other hand, if the mosquito is infected solely through man, why are sparsely inhabited countries, such as certain jungle tracts in India and Assam, very malarious? There mosquitoes are so many, and men so few, that the former must rarely have the chance of being infected through the latter. Again, on Dr Manson's hypothesis, it is difficult to understand why malaria varies so much in type with time and space.

ently they cannot multiply in the outside world. The microbes of cholera, typhoid, yellow fever, etc., and some other complaints, chiefly water borne, are able not only to exist outside the human body, but apparently can increase and multiply away from it. But they cannot thus multiply to an indefinite extent—at any-rate they cannot to an indefinite extent multiply away from water polluted by human excreta, for travellers in countries void of human inhabitants are not infected by them. They are saprophitic to a very limited extent only. Accordingly as diseases resemble in type syphilis or malaria, locality is of little or much importance. Syphilis, since it inhabits only the human body, is not affected by locality, and has therefore travelled everywhere, and is now of world-wide distribution. In this it is resembled by tuberculosis, measles, and other earth and air borne complaints. Cholera, yellow fever, etc., because more dependent on the outside world, are of more local distribution. Malaria, which is entirely dependent on the outer world, is strictly local; it does not travel all over the world, but infests certain well-defined tracts.[1]

Malaria and other saprophytic diseases, but chiefly malaria, have played a mighty part in the Natural and Political History of Man. Where malaria is mild, as in certain parts of North America, it is a grave deterrent, though not a complete bar to alien immigration. Where it is virulent, as in the Terai and on the West Coast of Africa, it acts as an absolute check to colonisation. In such countries, in exceptional instances,

[1] Bacteriologists have proved that the virulence of some diseases may be altered by changing the media in which the pathogenic micro-organisms exist. The same fact may be observed in nature. Syphilis, which has an unchanging environment, since it exists wholly in man, is very stable in virulence of type all the world over, and during long intervals of time—during intervals of time so long that any change of type must be attributed to the evolution of the race attacked, not to the increased or decreased virulence of the microbe attacking. Thus races which have long been afflicted by syphilis take the complaint mildly, but communicate it in a severe form to races that have had little or no experience of it. Phthisis, measles, whooping-cough, etc., are nearly as stable in type. But cholera, typhoid, and other water borne diseases, the microbes of which are subjected to the influence of the outer world to a greater extent, are much less stable. Malaria is exceedingly changeful. Subjected, as its microbes are, to all the influences of seasons and locality, it is mild at one time or in one place and severe in another. The microbes of disease multiply very rapidly, each microbe dividing into daughter cells. Bacteriologists have found that a character acquired by the mother cell (*e.g.* increased or decreased virulence) is frequently inherited by the daughter cells. It has been claimed that this constitutes an instance of

strangers from non-malarial regions may survive for years; but as a rule they perish early. In any case, they are unable to rear families. It is improbable therefore that the nations inhabiting the worst of these areas can have undergone the whole of their evolution within them; the death-rate of a non-evolved race being so high as to cause extinction, not evolution. They probably dwelt first within less malarious regions and underwent part of their evolution there. But now, after the sufferings of uncounted generations, after paying toll of millions of lives, they dwell secure in their fastnesses. They cannot be displaced by incoming hordes. They may be conquered by the superior weapons of civilisation, but even then, with the advance of their own culture, they must in time regain their freedom. Not for them is the fate of so many perished and forgotten races whose poor relics, rude implements and mouldering fragments of bone, alone attest their former existence, and tell of their extinction by stronger or fiercer invaders. They are safe for all time—unless, indeed, the march of sanitary science destroys malaria, still their scourge but now also their principal rock of defence.

If malaria and its congeners have played a great part, tuberculosis and other wholly parasitic diseases have played a greater. Being wholly parasitic and earth and air borne, they are necessarily diseases of crowded populations. Among scattered and nomadic peoples they tend to die out unless renewed from denser communities. During the Stone Age

the transmission of an acquirement. But the microbes of disease—unicellular plants and animals, each one capable of continuing the race—are germ-cells. The mother cell divides into daughter cells similar to itself; and if, before division, the mother cell is changed in any way the daughter cells inherit the change. The case is quite different as regards multicellular plants and animals. The germ-cells of these are not daughters of the somatic cells. When a character is acquired by a multicellular being, the change occurs in the somatic cells, which are then supposed by the adherents of the doctrine of transmission to affect in some way the associated germ-cells. But even did the change in the germ-cells happen, it could not of course be of the same kind as that in the somatic cells. For instance, exercise multiplies muscle-cells and renders them capable of enduring fatigue, a change which from the nature of the case cannot occur in the germ-cells. On the contrary, the latter are supposed to be so changed that their very remote cell-descendants, the muscle-cells of the child, inherit the peculiarity the parent acquired. It is clear, therefore, that the transmission of an acquirement is not by any means the same thing in unicellular and multicellular organisms. It is likely enough to occur in the former; it is wildly improbable in the latter.

they can have but little afflicted mankind. But for many thousands of years certain areas of the Old World, China, India, and the coasts of the Mediterranean have been thickly populated. In ancient as in modern cities the wholly parasitic diseases took toll of human lives, and age after age eliminated the unfittest. Our oldest records tell of plague and pestilence. With the advance of civilisation, as cities enlarged and multiplied, and the densely populated areas widened, the stringency of selection increased. The conditions became more and more favourable to the spread of infectious disease. Sanitation has done something in recent times, especially in England, yet even now, measles and tuberculosis, for instance, are so prevalent that no man escapes infection unless he be immune or death unless he be resistant. As a result, evolution against purely parasitic diseases has proceeded very far. We speak of the west coast of Africa as having a deadly climate. It is deadly to us who have undergone no evolution against malaria. But just as deadly is the climate of our great cities to races that have undergone no evolution against tuberculosis and measles. South Sea Islanders and Esquimaux, for instance, perish as surely in London as Englishmen in West Africa. To judge by analogy our ancestors of the Stone Age must have been as susceptible to measles and tuberculosis as Red Indians and South Sea Islanders—as little capable of dwelling in dense communities and consequently of achieving civilisation.[1] Our civilisation is therefore conditioned by our power of resisting certain infectious diseases, a power which arose during the advance of civilisation by a long and painful process of evolution.

[1] Professor Hamilton thinks that "there is good reason to believe, that instead of man being an animal very prone to tuberculosis, he is extremely insusceptible, otherwise it is difficult to explain how tuberculosis has not utterly decimated the human race." As a fact it has decimated it. So far from man being insusceptible to the disease, all races that have not been weeded out by it are extremely susceptible. He attributes the susceptibility to "a variation which took place far back in our history" which "has been propagated in the descendants ever since." But susceptibility to tuberculosis was plainly no variation; it was the original condition—a condition which the races of the New World to their undoing still retain. The real variation was insusceptibility. The most resistant, not the most susceptible races have departed rom the ancestral type. He added, "if you ask me in what the particular vulnerability consists, I should reply that, most likely, it resides in the epithelial coverings

Malaria infests a large portion of the earth's surface, but except in the south of Asia its habitat, mainly the great tropical forests, is thinly populated. Tuberculosis, the type and the worst of purely parasitic diseases, afflicts a larger and more densely populated area. From time immemorial it has ravaged the Old World. It is not unknown in African forests and Indian jungles, and therefore Africans and Indians have undergone some evolution against it; but it has longest or at least principally afflicted parts of Western Europe and Eastern Asia, where for thousands of years teeming populations have lived in houses so designed for warmth as to be more or less exclusive of light and air. Like all or most infectious diseases it was unknown among the scanty population of the Western Hemisphere till introduced from the Old World, whence came not only Old World diseases, but Old World conditions of life as well, teeming cities, air-tight houses and clothes, the best of all vehicles for the conveyance of infection. No gradual evolution as in ancient Europe was possible under such conditions. Extinction, swift or slow according to circumstances, was generally the only alternative. The unfit were so many and the fit so few that the aborigines dwindled. The Caribs and Tasmanians have gone. The Esquimaux and the Red Indians, the Patagonians, the Terra del Fuegians, the Australasians, the Polynesians are going—the greatest and grimmest tragedy in all human history.

Writing of the Spanish occupation of the West Indies, the late Professor Froude says: "The Carib races whom the Spaniards found in Cuba and St Domingo had withered before

of the body being too little resistant, too easily stimulated by external agencies, too readily penetrated by the parasite of the disease." But it is not to be supposed that all the races of the New World are weaker as regards their epithelial coverings than, for example, Englishmen. Moreover, when the parasite gets past the epithelial protections of Englishmen, the disease very often takes on a chronic form and frequently the sufferer recovers. Acute phthisis usually supervenes in the Polynesians and Red Indians, who are often men of very fine physique, and they seldom recover. It is a favourite notion that the complexion of the skin has some relation to the phthisical diathesis. Blonde people are supposed to be peculiarly susceptible. But the New World races are all dark, and Negroes and Papuans, who are very susceptible, are absolutely black. I take it that it is not in the skin or other epithelial coverings that the resisting power to zymotic disease resides, but in the phagocytes, whose special function it is to resist bacteria. It is probable that the so-called phthisical facies is not a precedent but a consequent of the disease.

them as if struck by a blight. Many died under the lash of the Spanish overseers. Many, perhaps the most, from the mysterious causes which have made the presence of civilisation so fatal to the Red Indians, the Australians, and the Maoris. It is with man as it is with animals. The races that consent to be domesticated prosper and multiply ; those that cannot live without freedom pine like caged eagles, or disappear like the buffaloes of the prairies. Anyway the natives perished out of the islands of the Caribbean Sea with a rapidity which startled the conquerors. The famous Bishop Las Casas pitied and tried to save the remnant that were left. The Spanish settlers required labourers for the plantations. On the continent of Africa were another race, savage in their natural state, which would domesticate like sheep and oxen, and learnt and improved in the white man's company." These sentences are typical of much that has been written concerning the decay of the New World races. Almost all writers unite in speaking of it as mysterious, and yet the facts are patent, are manifest to every observer on the spot. There is no more mystery connected with their decay than with the extinction of the dodo or the bison. It cannot be doubted that the New World races have suffered or are suffering extinction in consequence of the introduction among them of Old World diseases, and of concentrated Old World forms of alcohol. So much is quite beyond dispute, and these causes may be seen in operation over half the world at the present day—in North and South America, in Australia, in New Zealand, and in the islands of the Pacific, as well as in the Andamans, and several other of the oceanic islands of the Eastern Hemisphere. The sole mystery has lain in the circumstance that the races of the New World are less resistant to diseases of the non-malarial type than those of the Old World, and to that mystery I trust I have furnished a key. It is no question of freedom or of domestication, or even of civilisation *per se.* The continental savages of the Old World do not perish when brought into contact with civilisation. In India and Ceylon are tribes of an exceedingly wild type that have existed for thousands of years in contact with, and in the midst of, most ancient civilisations and very crowded populations. There is no conceivable reason why the Caribs should have been less capable of en-

during domestication or slavery or civilisation than the equally barbarous or even more barbarous negroes. But they perished, as other New World races are perishing, because, unlike the negroes, they had not been rendered resistant to the non-malarial zymotic diseases which the Spaniards introduced, and they would have perished had the Spaniards come among them as slaves, not as masters, and adopted their manners and habits of life, instead of forcing on them a change; for their islands lay in the very highway of the commerce that then sprang up, in the very path of infection. The natives of tropical America seem destined to survive. Defended like West Africans by virulent malaria they are not brought into close contact with Europeans, who cannot as colonists spread over the country in millions as in North America and Australasia. Consequently though these races have suffered greatly from measles, small-pox and other air-borne diseases, yet from tuberculosis, the most death-dealing of all diseases, they have suffered comparatively little. They owe their salvation besides to the smallness of their communities, and to the fact that the warmth of the climate renders pleasant the admission of plenteous air to their dwellings.

See then how the matter stands. Were acquirements heritable, races that had longest been afflicted by malaria or tuberculosis would be the weakest against them. In that case malaria, the microbes of which are mainly saprophitic, would destroy all human life within the areas infested by them. Tuberculosis, the microbes of which are entirely parasitic, mainly on man, would render all dense populations, and therefore all civilisation, impossible. But to rise to the full height of this great argument, consider in this relation the position of the Anglo-Saxon race. Its ancient home was in the British Isles, where under stringent selection it evolved for centuries against purely parasitic diseases. By rare good fortune its emigrants went mainly to lands undefended by virulent malaria and unprotected by evolution against purely parasitic diseases. Unchecked by malaria the Anglo-Saxons spread; smitten by parasitic diseases the aborigines dwindled. To-day the former hold so large and fertile a portion of the earth's surface that their predominance in the future is assured. They may be beaten in war; they may break into separate govern-

ments; but disease cannot now exterminate them, and their increasing numbers must secure the ultimate victory. The Latins, especially the Spanish and Portuguese, were less fortunate. They had the first start in the race, and they chose the seemingly richer tropics. But, checked by malaria, their diseases did not always exterminate the natives; and even where the latter did perish, as in the West Indies, their places had to be filled, not by pure Latins, but by half-breeds, and especially negro slaves imported from West Africa. All these communities have revolted, and Hayti is now a Negro Republic, foreshadowing the fate of our Indian and West African conquests. The Latins conquered, but in a real sense could not colonise. The experience of the Anglo-Saxons in other parts of the world is similar. They can colonise South Africa because it is not defended by virulent malaria, but they must share its possession with the natives, who have undergone evolution against parasitic diseases, and are not exterminated thereby. To West Africa and India, the homes of malaria, they can go only as conquerors. One day the natives, when sufficiently civilised, will certainly expel their masters. Alcohol, as well as microbic disease, has helped. The natives of India and Africa, who from very ancient times have manufactured solutions of alcohol strong enough to be poisonous, having suffered previous elimination, are not now exterminated by spirits. Moreover, the natives of India are protected by opium, the use of one powerful narcotic being a bar to the extensive use of any other. But given the opportunity, the natives of the New World drink to extinction. In the savage past races exterminated each other with the sword. Now they do it with disease. And the work done by disease is greater far than aught ever done by the sword.

Historians have chronicled how Jew and Saxon slew the Canaanite and Briton and entered into their inheritance. They tell us with vast amplitude of detail of little, and well nigh purposeless wars, of futile changes of dynasties, and so forth. They give us low gossip concerning kings and queens. But of this momentous march of disease, which has for ever changed the political face of the world, they tell us nothing. Biologists and philosophers have expended vast learning and labour in tracing the evolution of hoof or of tail. They are nearly all

agreed that evolution is caused solely by the elimination of the unfittest. For thousands of years diseases due to micro-organisms or to the use of powerful narcotics have been the main cause of human elimination, and therefore of human evolution. But of this great process and its tremendous consequences biologists are silent. Anthropologists have carefully differentiated the races of mankind, recording minute variations in size, shape, colour, hair, and features. But they have quite ignored the most important of all human differences. Medical men have a monumental literature on disease as it affects the individual; on disease as it affects the race they have published scarce a volume. But no theme more august could have engaged the pen of any writer. They have examined man through a microscope, as it were, but a wider view is also necessary. Elaboration of detail is imperative; but by an extended survey much may be learned which the study of minutiæ cannot furnish.

Heredity, obviously important from a scientific and theoretical standpoint, is not without its practical bearings.[1] No philanthropist who has at heart the well-being of aboriginal races can afford to neglect its teachings. The savages of the Old World, the negroes for instance, are capable of civilisation; in other words, having undergone evolution against parasitic diseases, they are capable of persistence under the ordinary conditions of civilised life. Not so aborigines of the New World.

The success of African missionaries has no counterpart in the Western Hemisphere, at any rate in such parts of it as are not affected by virulent malaria. Many observers in Polynesia have noted that the establishment of a mission is frequently followed by the more or less rapid extinction of the congregations. When we remember that all missionaries build churches and schoolhouses, wherein their flocks are crowded, and insist on the wearing of clothes, the result

[1] It concerns the question of temperance reform very closely, but on that I have written at length elsewhere, and am glad to have escaped with my scalp. Reformers are proverbially energetic. Gentlemen who have not thought it worth while to contest my premises have discovered that I am grievously lacking in good sense and ordinary morality. One reverend gentleman compared me to Satan and Judas Iscariot, with a judicial leaning to Satan. He suggested, however, that I might with advantage increase my resemblance to Judas by imitating him in the last act of his unworthy life.

is not surprising. The natives are placed under conditions the most favourable for the spread of zymotic disease. Is it too much to hope that natives may be converted in the open air, and that the wearing of garments is not a necessary prelude to eternal bliss? Occasionally clergymen with incipient phthisis adopt a missionary career in the double hope of benefiting themselves physically and the heathen spiritually. In the pure air and sunshine of Pacific Islands they often recover. But phthisis is an infectious disease, and Pacific Islanders are less resistant than the least resistant of Europeans. Under such circumstances we may reasonably expect a religious minded pastor to manifest gratitude and admiration for restoration to strength at the very moment when his flock, mysteriously smitten, most needs his ministrations. The situation in the South Seas is pitiful, but occasionally it is not devoid of a grim humour.

As I have already intimated, it is not possible to consider the whole question of heredity within the limits of a magazine article. The subject is too big even in its medical aspects. I trust, however, even in the foregoing sketch, I have not altogether failed in my endeavour to demonstrate its importance. To sum up: it is highly improbable, from developmental reasons, that acquired characters can be transmitted; no single instance of such transmission has ever yet been proved; no body of men possess evidence on this point so overwhelming as medical men, but the evidence in their hands has generally been neglected or misunderstood; properly used, it supplies matters of the highest importance concerning the Natural and Political History of Man.

Notes of Cases

INTUSSUSCEPTION AND FAILURE

By J. TREGELLES FOX, M.R.C.S.Eng.,
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As we learn individually from our own failures, or even the mistakes of our teachers, so I believe we could reap collective benefit if we entered into a league and compact to record, or at least report, all *unsuccessful* cases.

I have just had a case of acute intussusception of the cæcum in an infant under five months, performed laparotomy and found it irreducible on the fourth day, resected the bowel with some difficulty, the patient surviving five hours.

J. M'K., fine healthy boy of nearly five months, on the breast.

June 8.—Vaccinated.

June 14.—Bowels acted as usual.

June 15.—A trifle fretful, crying and starting, and bowels did not act, so the mother gave, on

June 16, a teaspoonful of castor-oil, which acted in the afternoon. There was healthy fæces at first, followed by a considerable blood-stained mucus discharge; and the child evinced little taste for its (natural) food.

June 17.—Slime and blood only, passed frequently. Pain, and vomiting, first of milk, then "green stuff."

June 18.—I was sent for and treated it for gastro-enteritis from the irritation of castor-oil. The abdomen was not swollen nor hard, there was no sign of chest mischief, the vaccination was normal, not inflamed, and no pyrexia. There was a frequent *distressed* cry.

June 19, *at noon.*—Seen again, evidently worse, mottled skin, dry, furred tongue. A little of the same discharge passed and occasional vomiting, with anorexia. A tumour was felt on the left side of the centre of the abdomen. No obvious dulness. Temperature normal. Intussusception diagnosed. 3 P.M.—Morphia sulph. $\frac{1}{200}$ grain, hypodermically. Enema of warm water, which brought a very little bloody mucus only, given by pipe and funnel, with pressure of about 3 feet vertically. 7 P.M.—Morphia $\frac{1}{100}$ grain. Another enema which did not enter well. The child seemed much easier after the first, but not after the second enema. Took the breast with avidity and retained it, sleeping peacefully thereafter till 2.30 A.M.

June 20, 4 A.M.—Seen. The same distressed cry. Has taken the breast again with relish once or twice and not vomited. A very little of the same semi-solid mucus was passed. Morphia $\frac{1}{100}$ grain. Consultation sought with a view to operation. 5.30 *to* 6 A.M.—Chloroform administered, on consultation with a medical brother, and a copious injection of oil, then water, given by gravitation whilst under anæsthesia.

About ten ounces entered, but squirted out when let go. A lump was felt in the left iliac fossa, but the right rectus bulged most. 11 A.M. *to noon.*—Laparotomy performed in left semilunaris under the idea that the left colon or sygmoid was probably the seat of the obstruction. The tumour was found high up under the liver, and was with difficulty brought out by tracing from the sygmoid. The vermiform appendix (not at first recognised) and a very congested piece of small intestine, the latter tightly held by mesentery, entered together into the upper part of the tumour, viz., the large intestine. Traction on the vermix and small intestine, combined with compression of the strangulated mass felt through the walls of the large bowel, effected no reduction, and soon the colon split near the constriction, revealing an interior strangulated portion about 4 inches long by ¾ inch wide, of a port wine colour. This and the damaged piece of colon were removed, the small and large intestine being stitched to the skin in the wound, the peritoneum flushed and closed with sutures, except where a drainage tube was left in.

There was much shock; it seemed as if the child would not live through the operation. There was a partial reaction afterwards. He was fretful, but not thoroughly conscious; at 2.30 P.M. he was perspiring. Morphia, $\frac{1}{200}$ grain, was given. He died after vomiting a little blood at 4.35 P.M.

Remarks.—The facts of this case speak for themselves, and re-echo the old lessons on early diagnosis, the treacherous period of repose and effect of morphia, the duty of prompt operation, and futility of repeating enemata more than once. But I would offer to my fellows of the rank and file, who may at any time be called upon to undertake a like responsibility out of reach of a specialist whose practice "keeps his hand in" the abdomen, one or two further remarks:—

1. The great benefit of morphia—lessening pain and spasm, probably preventing further invagination, and even enabling the patient to retain some nourishment.

2. I will set down the dose I used, as the rules of the books are difficult to apply to an infant under five months. $\frac{1}{200}$ grain was given, then half that amount after four hours, and the effects were watched in diminishing the pupils (in darkish room), and in somnolence. If there was restlessness and more

pain, and the pupils had dilated, I felt justified in repeating the dose. In all $\frac{3}{200}$ grain was given in twenty hours before the operation and $\frac{1}{200}$ after. The latter had probably better have been omitted. The sulphate was used, a quarter grain being dissolved in about 50 minims of water, and the syringe adjusted by screw to deliver 1 minim, or $\frac{1}{2}$, etc. It was a strong male infant of twenty weeks. I did not combine atropine with the morphia, which would, I imagine, mask the effect upon the pupils.

3. These (and many other) abdominal cases appear to improve under morphia, rest, and withdrawal of food, yet *cannot possibly be so cured.* The symptoms are most deceptive. I should like to know what the first symptoms of recovery are; where, for instance, inflation by water or air has caused reduction. Retention of food, and absence of vomiting, and relief of pain cannot be relied upon. I think that the absence of *fresh* fæces coming through, or occasional attacks of restlessness, with the same *distressed cry* or the presence of *hiccough*, should warn us not to lose any time in operating. Rise of pulse or of temperature or increase of weakness are signs we should *not* wait for, as by that time septic absorption and other complications will have reduced the chance of success to a minimum. If those who have been so fortunate as to witness cures of intussusception by inflation or hydrostatic enemata would describe the first reliable signs of improvement it would be helpful. But as I understand the most recent teaching, mere stretching and stasis in even healthy bowels may cause constitutional symptoms, and even fatal effects, from septic absorption; which rather militates against the idea of doing much good this way.

4. As to the operation I have not much to offer, except to be always ready for it; to do it as soon as possible; to stick to the linea alba; and to have ready, as resection may be necessary, a few decalcified bone or glass tubes of different diameters to which to fix the upper segment of bowel temporarily till its distension is relieved, also clamps.

NOTE ON VACCINATION

By G. DICKSON, M.D., F.R.C.S.Ed.

HAVING been frequently annoyed by the failure of calf lymph to take effect, due, I believe, to the glycerine (added thereto for preservative purposes) rendering it liable to be rubbed off, I tried the following method with complete success.

Make the usual scarifications, rub in the lymph, then apply a piece of guttapercha tissue about the size of a halfpenny, keeping it in situ by means of cross pieces of rubber adhesive plaster. Give directions for the removal of the protective on the fourth day after vaccination.

By this simple plan time and disappointment are saved, as there is no need for waiting until the lymph dries, and no failures have resulted since adopting it.

Public Health

The Glasgow Conference on Tuberculosis

CONFERENCES on tuberculosis are more or less in fashion for the present. But hitherto each conference has had its own special function in the laborious conversion of medical ideas into administrative forces. Glasgow, impelled no doubt by the problems special to its life as a great city, chose to confer with the delegates of other local authorities on two particular aspects of the tuberculosis problem, namely, meat-supply and milk-supply. The Conference was held on the 24th of April. Some 300 persons took part in it. They were there not as private individuals, but as delegates from local authorities. This is, so far as I am aware, the first time that any such representative gathering has met for the sole purpose of exchanging opinions and passing resolutions on a definite public health question. A little time ago, the Municipality of Edinburgh met the Medical Colleges and sought their guidance. Glasgow, assuming the practicability of improved administration on the lines already understood, summoned the local authorities to help one another in a concerted attack. What was the plan?

To begin with, a single day was too little for even the limited questions raised at Glasgow. The organisers of the Conference propounded for discussion only this—"the prevention of tuberculosis in its relation to meat and milk supply." Naturally, a subject that occupied some years and two Royal Commissions could not be exhaustively dealt with in five hours.

But that is not quite the right way to look at the case. The purpose was rather to focus the results of research for the ends of administration in the burghs and rural districts of Scotland. And on that aspect every man there had some opinion of his own and some right to have an opinion. The discussion soon revealed where the strong opinions were.

A few remarks from the Lord Provost made quite clear why the Conference was called. Glasgow was a great city; it depended for its food supply on the neighbouring counties; it had spent great energies on improving houses and streets; it wanted help in controlling more effectively the purity of its meat and milk. Bailie King, Chairman of the Tuberculosis Committee—surely a suggestive title for a municipal committee—sketched the proposals for veterinary inspection of byres, for the improvement of meat inspection, etc. Bailie Dick, Chairman of the Health Committee, supplemented the case from Glasgow's standpoint. Then a paper was read on "The Regulation of Meat and Milk Supply." A discussion ensued; two resolutions were ultimately passed; a supplementary resolution on foreign meat and milk was assented to; and this was the morning's work.

The essence of the resolutions was this: first, that in order to make the new Dairies Order of 1899 effective in practice, each local authority should provide for the regular inspection of its dairy cattle, by the appointment of a veterinary surgeon for the purpose; second, that in order to secure uniformity in the exclusion of tuberculous animals from the meat supply, the local authority should avail themselves of the powers conferred by the Public Health (Scotland) Act, 1897, sec. 43, whereby definite places may be provided for the inspection of animals slaughtered for food purposes. As a provisional standard for condemnation of carcasses, the recommendations of the last Royal Commission were to be taken.

There was no difficulty in securing assent to much of what is implied in those propositions. But the discussion did not go far before the hard facts of economics made themselves very visible and audible. On the merits of the case for infection or non-infection, lay opinion is, as it were, only raw material for the scientific method; but on the merits of profit and loss, of the immediate economic results of enforcing these provisions, lay opinion is precisely what the administrator must hear. When, therefore, one large raiser of stock and seller of meat declared that infection from milk was "all humbug," he merely gave passionate expression to his ignorance; when, however, he declared that inspection of every carcass by a competent inspector was detrimental to trade, impossible in practice, and

unnecessary, he was on more arguable ground. But as he produced no fact of a kind to prove his strong assertions, his opinions, however representative of the scared trader, cannot be accepted as a reason against the enforcement of the Act. This gentleman's contention was that "definite place for the inspection of animals slaughtered" was the same thing as a Meat Clearing-House. That was what he wanted; let there be a place where he could send his carcasses (uncertified, of course), and if the carcasses passed the Clearing-House Inspector, let that "clear" the seller from any risk of further prosecution. This is a deceptively plausible way of putting the case, and although the propounder of the "clearing-house" got only some seven votes, there was some indication that the vote did not fully express the opinions in favour of a clearing-house.

Now let us be quite precise on this matter; for this "clearing-house" question has been disputed for years between traders and municipalities. The meat clauses of the Act of 1897 were expressly framed to embody what was good in the conception of a clearing-house, and to make impossible what was bad. A clearing-house, as the gentleman in question understood it, is a place where all carcasses, or parts of carcasses, that are sent to (say) a city for consumption must be submitted for inspection in the City Clearing-House. Carcasses may be sent from Aberdeenshire to Glasgow. The contention is, that if Glasgow has a clearing-house, that will be a sufficient protection against the importation of bad meat. In point of fact, it has been found to be no protection. Nor that only! It has actually encouraged the importation of bad meat, and made more difficult its arrest or detection. These are curious results. But there is no mystery about them. Frequently it is impossible even for a skilled observer to detect tuberculosis in a carcass that is not accompanied by its viscera—lungs, liver, pleural membrane, bowels, etc. But these were not sent to the clearing-house. On the contrary they were apt to be carefully destroyed. They could not well be sent long distances by rail. Consequently the "inspection" of such meat was worth little. Then, if a carcass were stopped anywhere near Glasgow, the explanation given—if the meat was bad or doubtful—was that it was merely on the way to the clearing-house. To meet these objections, the 1897 Act made it obligatory that where "definite places" for inspection were fixed by a local authority, "no carcass shall be submitted for examination . . . unless as a whole carcass, including the thoracic and abdominal viscera, in such manner that the examiner shall be readily able to satisfy himself that the organs are those of the carcass under inspection." In other words, the conditions are as nearly as possible the conditions

present in the ordinary abattoir or slaughter-house. To confuse this with a "clearing-house" where unrelated bits of meat are to be "passed," is to darken counsel with words. The Conference by an overwhelming majority decided in favour of inspection in terms of the Act, and against the conception of a clearing-house.

But there was another issue. If we are to be so exacting about home meat and milk, why not be equally exacting about foreign meat and milk? The Rev. Dr Gillespie, himself a famous breeder of Galloway cattle, estimated that Britain takes from abroad milk equivalent to that yielded by the 166,000 odd cows of the six southern Scotch counties. Perhaps that is so. Perhaps it is equally desirable to be protected against the tuberculosis of foreign meat and milk. But after listening to a good deal of talk, some of it highly rhetorical, and after arguing with a young farmer whose economic troubles seemed to be due mainly to foreign importations, I was unable to detect one single practical suggestion for the inspection of foreign milk or meat. A good deal can be done, no doubt. Even a "clearing-house" might here have something in its favour. But from the vehemence that marked the expression of the resentment against possible foreign tuberculosis, I gathered that some men would be satisfied with nothing less than inspection abroad by British inspectors. Indeed this was actually proposed by one man "as a rider!" Possibly the day may come when international law will make practicable a detailed meat-and-milk treaty; but, for the present, to suggest that such a far-off possibility is on the same footing of practicability as definite clauses in a Scotch Act of Parliament and a Provisional Order is, whether intended or not, another way of proposing to do nothing. Besides, the food question of these Islands cannot be settled by any slap-dash method that happens to suit the economic interest of a particular class of traders. Apart from this, no actual facts were produced to show whether foreign meat and milk had ever been thoroughly tested for tuberculosis. The "desirability" of protecting the consumers against this possible source of infection was readily assented to; but the method was yet to seek. Perhaps the many Conferences of Nations—the Imperial Authorities—may hasten the day.

This was undoubtedly the main discussion of the Conference. But some good work was still left. A paper was read by a farmer on "the best means of housing cows in country and urban districts." This gentleman, Mr John Speir, is recognised as a practical expert on matters of cow-keeping and byre-construction. He gave many practical hints of the highest value. The most novel and original point, however, was his

observation about the "winter coat" of cows. He has noticed that in the autumn, cows (presumably those that live partially in the open air, but he did not exclude others) develop a thicker covering of hair. If they are kept in byres at a low temperature, say between 40° F. and 50° F., they retain their thick coat, they do not suffer from "weeds," and they yield an amount of milk not perceptibly less than they do in warm weather. If, on the other hand, they are kept in warm byres at say 70° F. to 80° F., they lose their winter coat, they more readily take "weeds," and, although they yield a little more milk, they lose in general health. This is a very striking biological fact. Coming from a practical farmer, it takes front rank in importance. For if this is true, the medical case for ventilation is made out. Hitherto, one of two alternatives faced anyone that meant to ventilate a byre on "human" lines; either he must cool the cows, so reducing seriously the milk yielded, or he must heat the air, so increasing the outlays and practical difficulties. Mr Speir has shown that, given reasonable conditions for the nurture of cows, ventilation may be made efficient without either heating the air or cooling the cow. Further observations are needed here. What the facts are in town byres, where cows usually say farewell to daylight whenever they enter the byre, is a matter for experiment. In any case, this one fact was worth the Conference. It meets in a scientific way the central difficulty of byre ventilation, and it is adduced by a man familiar with all the conditions—economic and biological—of the cow-problem. This paper will be a valuable addition to the study of the cow in the more or less pathological conditions of her milk-yielding life. I should add that Mr Speir laid emphasis on the value of light for cows.

The "Tuberculin Test" and "the Establishment of Public Abattoirs" were discussed, but by that time the interest of the Conference had ebbed somewhat. Tuberculin and its applications are somewhat two-sided; not because Tuberculin is not a reliable agent, but because, for the present, its application is inseparable from the question of compensation. Matters must ripen a little.

Of the Conference as a whole, there is this to say:—the discussion was mainly confined to non-medical and non-veterinary administrators. Why this should be, I do not well understand. Many medical men and many veterinary men were present. Many of them have expressed themselves on the question elsewhere. Possibly they preferred to take this occasion of hearing the practical lay opinion of their local authorities. Yet after all, one cannot but remember that the tuberculosis problem is primarily medical; that the driving force of the whole

movement for extirpation is the impulse to save human life from a preventable disease; and that, if the medical administrator fails to assert his entire practicality in conference with so-called "practical" men, he is, to that extent, losing the tide. On the other hand, it must have been gratifying to the medical men present to learn that, after all deductions for individual vagaries, the local authorities of Scotland give every sign of at last taking the question seriously.

Meetings of Societies

Edinburgh Medico-Chirurgical Society

THE seventh meeting of the seventy-ninth session was held on Wednesday, 2nd May, Mr A. G. Miller, the President, in the chair.

Mr Cotterill showed a patient after laminectomy for paralysis of two years' standing. The disease of the spine had not begun till the unusually late age of thirty-six; for two years there had been certain spinal troubles and for six months absolute motor and sensory paralysis, the latter extending as high as the anterior superior spine. Ankle clonus was much exaggerated, there were two large bedsores on the buttocks, a purulent cystitis and incontinence of urine.

Treatment by extension and counter extension was tried for a few weeks with slight improvement. Mr Cotterill then removed the spines of the eighth, ninth, and tenth dorsal vertebræ with the corresponding laminæ. There was no perceptible caries at all. Marked bony pressure on the cord was found, the spinal canal being narrowed by exostosis to the diameter of an ordinary slate-pencil. In sixteen days improvement began, first sensory, then motor. A year had now elapsed and patient was able to walk about by aid of a staff, water was passed naturally, the bowel was under control, and sensation had completely returned.

Mr Shaw M'Laren exhibited a specimen of carcinoma of œsophagus and stomach after gastrostomy performed some months before patient died. It showed the narrowing of the œsophagus, and that the cancer had eaten its way through to the trachea.

Mr M'Laren also showed a right and a left carpal scaphoid, both of which had been fractured by a fall from the roof of a house. There was no other fracture, and patient died of laceration of the brain.

Dr W. G. Aitchison Robertson read a paper on "The activity

of the Saliva in diseased conditions of the body." He said that many teachers have been in the habit of stating that the amylolytic power of saliva is feeble, and that any slight transforming action which it possesses is soon brought to an end by reason of the acidity of the gastric secretion. Dr Robertson said it could not be sufficiently inculcated that the converting power of ptyalin is extremely active, and transforms a relatively enormous proportion of starch into sugar, and this within a very brief period. It has been calculated that 120 grains of ptyalin are secreted each day as compared with 40 grains of pancreatin, and it has been shown over and over again that no free acid appears in the stomach until, at earliest, three-quarters of an hour after a mixed diet has been eaten. Consequently during this period the salivary ferment can exert its uncontrolled influence on starch in the stomach. The presence of a minute trace of free acid still further increases the activity of ptyalin. During the first period of gastric digestion the only free acids which may be produced are lactic or acetic, and so, under normal conditions, amylaceous matters undergo a very thorough digestion in the stomach. The salivary ferment is, however, destroyed by the gastric juice during the period of full proteid digestion. Thus only the more resistant portions of starchy foods as, for example, imperfectly-masticated potato, undercooked vegetables, rice, sago, tapioca, etc., may require a further disintegration and solution than they receive in the stomach. This they obtain when they come under the action of the amylopsin of the pancreatic secretion. These facts dispose of the too prevalent idea that salivary digestion takes place only in the mouth and for a few minutes afterwards in the stomach.

It has been demonstrated by several observers that there is a daily variation in the diastatic action of saliva. Thus, on getting up in the morning the converting power of saliva is strong. It then falls towards and after breakfast, then rises slowly and reaches the maximum before the mid-day meal. After this it again falls rapidly and remains low for two hours subsequently. Towards the evening meal it rises slowly, and this is succeeded by a gradual fall. So as to eliminate this variation in the experiments, Dr Robertson always collected the saliva at the same period during the evening. The experiments were performed in the following manner. Each individual was instructed to wash out his mouth thoroughly with slightly warm water, and during the succeeding half hour all the saliva which he secreted was received into a vessel and its quantity measured. Two cubic centimetres of the saliva were then mixed with ten cubic centimetres of starch mucilage at a temperature of 38° C., and the mixture was then kept at this

temperature for ten minutes. At the end of this period the condition of the starch present was noted, and further action of the ferment was prevented by rapidly boiling the mixture. The amount of sugar which had been formed by the ptyalin was then estimated by titration against standard Fehling's solution.

The activity of the salivary ferment was investigated in ten healthy adults, and in almost every instance the amount of sugar produced by the action of 2 c.c. saliva on 10 c.c. starch solution for ten minutes reached about ·1 gramme, and in some cases this figure was greatly exceeded. In children under five the average amount of sugar formed is ·077 gramme, the average figure all over from 16 months to 13 years being ·078.

More than 100 cases of disease of various kinds were investigated in order to see if the activity of the salivary ferment were altered, and it was found that the amount of sugar formed in such cases varied from nil to ·128 gramme. In chronic gastric catarrh the figure varied from ·078 to ·1 gramme. In acid dyspepsia the amount of sugar formed is above the healthy average. In ulceration of the stomach the amount of sugar formed is generally only slightly below the normal average. These general observations agree in the main with the conclusions arrived at by two Italian investigators, Petterutti and Ferro, who state that the greater number of gastro-intestinal disorders are associated with a decrease in the amount of ptyalin. In one case of great dilatation of the stomach, the salivary ferment proved to be almost absent or at least inactive in the saliva. In cirrhosis of the liver the amount of sugar is by no means reduced, and even in some cases it is greatly increased.

In the group of lung diseases generally the salivary ferment is fairly active, and on an average ·087 gramme of sugar was formed. In the cases of phthisis which were examined the sugar formed almost always reached the standard quantity. The pneumonia cases showed that the amylolytic action of the saliva is higher than the normal during the period preceding the crisis but lower after the crisis.

In the large group of heart cases the saliva retains its usual composition, and the amount of sugar formed hovered at or about the normal limits.

A larger proportion of subnormal cases was met with in the group of nervous diseases, forty-one per cent. giving a proportion of sugar lower than the normal average. In one case of cerebral tumour the saliva, though copious in amount, contained practically no converting ferment, whereas in a case of locomotor ataxia, though the secretion was equally copious,

the converting ferment produced the large amount of ·111 gramme sugar.

Of three cases of Addison's disease examined, the saliva of two showed marked deficiency in its power of starch digestion, while the third exceeded the normal limit.

Taken all over, the group of renal diseases showed a lower average than the normal. In 55·5 per cent. the quantity of sugar formed was considerably below the average. In diabetes, on the other hand, the saliva has a very active converting action. In three out of four cases examined the average proportion of sugar formed was much above the standard figure. The saliva in these cases contained no traces of sugar.

In simple anæmia the converting ferment seems to be present in the saliva in its normal amount. If, however, the anæmia be associated with dyspepsia, the average is subnormal.

In subacute and chronic rheumatism the ferment shows no great variations from the normal.

In general febrile conditions the secretion of saliva is greatly reduced in amount, and this reduction increases *pari passu* with the increase in temperature. It is important to note that this scanty secretion seems to be possessed of enhanced amylolytic power.

Passing to the consideration of the *amount* of saliva secreted in various diseased conditions of the body, and taking the normal amount secreted by healthy adults as from 10 to 15 c.c. in half an hour, it was noted first that in infants and children the amount of saliva poured out is relatively much greater than it is in adults.

In the case of females the secretion seems to be generally a little more copious than in males.

In most cases of acid dyspepsia the secretion is above the normal in amount. This may be due to reflex irritation of the salivary glands.

In chronic gastric catarrh the amount secreted is hardly as a rule up to the average. In ulceration of the stomach the same deficiency of secretion is met with.

In those cases where diarrhœa or ascites are present the secretion of saliva is often far below the normal amount.

In bronchitis and in the early stages of pneumonia the volume of saliva which is secreted is generally up to the full average, and may even be beyond it. In the case of chronic phthisis, on the other hand, the secretion of saliva is always very scanty.

In the large class of heart affections of a grave nature there is an almost constant diminution of the secretion.

In affections of the spinal cord the amount of saliva secreted

reaches the highest average, and even surpasses it, as, for example, in locomotor ataxia.

In hemiplegia the same excess is found. The reverse is found generally in the class of cerebral tumours, where the saliva is very small in amount.

In chronic Bright's disease as a rule the salivary secretion is scanty. This may account for the very fœtid odour of the breath so often present in such cases. In diabetes, of course, the saliva is very scantily secreted.

In simple anæmia, in the chronic forms of rheumatism, and in three cases of Addison's disease examined, the amount of saliva was subnormal.

In fevers generally, when the temperature is at all high, the secretion of saliva is lessened in amount, but, as already noted, its amylolytic power is increased.

In very slight degrees of fever, however, the normal amount may be produced.

It might be thought that where a large amount of liquid saliva is secreted the ferment would not be in so large an amount as it would be in an equal volume of saliva where its secretion was scanty. Such an opinion, though true in the main, is not in all cases borne out by a consideration of the facts observed either in the healthy or diseased conditions. In many cases where the secretion is scanty, the amount of ptyalin is also small; while in other cases where the secretion is copious, its proteolytic power is also great.

Dr Robertson pointed out that it was an easy matter for anyone to satisfy himself by a simple examination of the saliva as to whether unchanged starchy food ought to be given to an invalid or not. If the secretion be very scanty, as, for example, in pyrexia, then it would be quite out of the question to think of administering such a food. If, on the other hand, the secretion be present, but there is doubt as to its amylolytic power, then the experiment is readily performed. A small quantity, about 10 c.c., of starch mucilage is to be mixed with 1 or 2 c.c. of the saliva in a test tube, and the whole is to be kept at a temperature of 100° F. for five or ten minutes. This can readily be accomplished by immersing the tube in a large body of warm water, and taking the temperature of this by means of a clinical thermometer. At the end of this period the mixture is to be boiled. If a few drops of this be now poured on to a plate, and a drop of a weak solution of Iodine be added, the degree of starch conversion will be at once shown. If the ptyalin has had no action, then the starch is merely coloured purple; but if the ferment has been active, then a reddish-purple or red-pink colour is produced,

depending on the intensity of the action. If the ferment has been energetic no colour is produced, the whole of the starch having been converted into the colourless form of dextrin achroo-dextrin). The latter result is seldom obtained, and it is generally found that a more or less red-purple colour is produced, indicating that only part of the starch has undergone conversion. The redder or pinker the colour the greater has been the power of the ptyalin.

Next, a small quantity of Fehling's solution is boiled in a test tube and one drop of the mixture is added. If no precipitation of the suboxide of copper takes place a second drop is added, and so on till a precipitate is produced. Such a proceeding gives a very rough estimate of the amount of maltose produced, and consequently of the intensity of the salivary ferment. These two tests are so simple and easily performed that no one need neglect the examination of the saliva in a doubtful case.

It is evident that a full appreciation of the amylolytic power of saliva is of immense value in considering the dietetics of disease. In those cases where the salivary secretion is extremely scanty or even in abeyance, as, for example, in the exanthemata, or in dilatation of the stomach, where the saliva contains almost no ptyalin, or again in cases where the secretion has been inhibited by drugs such as morphine or atropine, it is evident that ordinary starchy food ought not to be allowed. If given at all, starch must be in the first place rendered soluble by treatment with some of the artificial digestant agents as extract of malt, zymine, taka-diastase. These facts are insisted on in every reliable work on Dietetics, but in spite of this they are generally ignored in practice. One authority, for example, recommends farinaceous foods in gastric dilatation—a disease in which the proteolytic action of the saliva is very feeble or even absent. In conclusion, Dr Robertson advocated a systematic examination of the saliva in all diseases of the gastro-intestinal tract, as well as in other diseases where ordinary starchy food is found to disagree with the patient.

Remarks were made by Drs Harvey Littlejohn, George Hunter, Jas. Ritchie, and Church.

Dr Claude B. Ker read a paper on "Cross-Infection, so-called, in Fever Hospitals." He said that wherever large numbers of children are congregated together, whether in schools or in hospitals, there is always a risk that infectious disease may break out among them, and that it seemed a perfectly logical deduction to make that this risk will be materially increased if the place in which they are congregated happens to be a hospital in which infectious disease is treated.

It was thus not surprising to find that many parents have a strong prejudice against sending their children into a fever hospital lest they should, whilst resident there, contract another infection, and in this prejudice they were not infrequently encouraged by their family physician. Dr Ker was afraid the Edinburgh Fever Hospital had not a very good reputation as regards what is ordinarily known as cross-infection, and he proposed to consider how far that reputation was or was not deserved, and at the same time to remove some misapprehensions which apparently exist as to the nature and causes of this not infrequent occurrence.

By cross-infection is usually understood the contraction by a patient of some infection other than that for which he was originally admitted, and it is always presumed that the second infection is derived, directly or indirectly, from patients who are being treated for that infection in some other part of the hospital.

Last year the Edinburgh Fever Hospital suffered rather more than usual as regards cross-infection. Of 1034 cases of scarlatina 38, or 3·67 per cent., took measles; of 331 cases of measles 8, or 2·4 per cent., took scarlatina; of 147 cases of diphtheria 4, or 2·7 per cent., took scarlatina; and of 1034 cases of scarlatina 4, or ·38 per cent., took varicella. Of 472 patients admitted with infections other than the above, none took any other disease. The views most generally held by outside critics were stated as follows:—(1) the second infection is contracted from patients suffering from that disease in other parts of the hospital; (2) that in many cases it is carried by the air; (3) that it is carried by the officials from one ward to another; (4) that the nurses mixing at meals contaminate each other, and so the patients under their charge; (5) that wards which have been used for the treatment of one disease are not properly disinfected before another disease is nursed in them.

All these *alleged* causes depend on the assumption that the second infection is carried from patients in other parts of the hospital. Dr Ker was prepared to admit that such might be the case in certain rare instances, but he asserted that this method of infection was not at all a common one, and he pointed out that outbreaks of measles occur not infrequently in the magnificent hospitals of the Metropolitan Asylums Board, where measles are not treated at all.

As regards the carrying of infection by the air, it was interesting to note that the measles wards in the Edinburgh Hospital are below those used for typhoid, and may be almost said to ventilate into them by a staircase guarded only by a swing-door. The scarlatina wards are on the same flat as those

for typhoid; and a similar swing-door, used frequently by the medical and superintending staff, alone separates them. Yet not a single typhoid patient in the last four years, though at least 400 children must have passed through the wards in that period, had developed either measles or scarlatina. Small-pox, that infection which of all others is most regarded as aerial, had been nursed in the narrow limits of the hospital courtyard without accident. On the other hand, Dr Ker had seen two cases of true cross-infection within the last few years which he was convinced were due to aerial infection. Both were cases of measles, both were treated at different times in the same private ward, and both contracted scarlatina. The ward which they occupied is in the angle of the courtyard, and its window faces, at the distance of a few feet, a window of a scarlatina convalescent ward. When both windows were open, it was easy to see that infection could hardly be avoided. The case, however, was altogether an exceptional one.

As regards the question of infection being carried by the staff, Dr Ker admitted that there must always be some slight risk of it. A few cases of diphtheria had been so infected with scarlatina, a disease to which they seem to be peculiarly susceptible. That nurses occasionally infect each other in the dining-room was true, but there was no reason to believe that they had time to carry their disease to their patients before they, the nurses, were isolated. In the new hospital the scarlatina nurses are to be kept quite separate from the rest.

As for the last alleged cause—want of proper disinfection of wards—Dr Ker thought the disinfection as carried out in the Edinburgh Hospital quite efficient, and that any outbreak which may occur in wards which have been used for another disease at some previous period can always be accounted for in a much more obvious manner. Dr Ker was of opinion that most of the so-called cross-infection was not cross-infection at all, that is, it did not come from other parts of the hospital but from outside. The *real* cause was imported infection, and it might be introduced in a variety of ways.

Firstly, it might be due to a wrong diagnosis made outside. There are often difficult cases of scarlatina and diphtheria, or of scarlatina and measles, which may be confusing. The hospital physician could check the diagnosis on admission, but in many cases no signs of any disease at all are visible on admission, and if he did not accept the notified diagnosis, which in the vast majority of cases is of course correct, it would be necessary to isolate nearly a fourth of the admissions, which is manifestly impossible.

Secondly, it might be due to a patient being admitted with

two diseases, only one of which has been diagnosed. That two infections can exist simultaneously is not only possible but it is comparatively common. Dr Johnston, in his report of the Glasgow Fever Hospitals for last year, noted no less than seventy-six cases admitted with two concurrent infections, and three in which three infections co-existed.

Lastly, it is due to the admission of patients suffering from one disease and incubating, another. The second infection, in the great majority of cases, either measles or varicella, is developed some days after admission. This is by far the most common of the causes of so-called cross-infection, and it is quite impossible to avoid it. The outbreaks of measles among the scarlet cases in the Edinburgh Fever Hospital last year were due in both instances to cases of this kind. Twenty-seven cases of this sort were admitted to the Glasgow Fever Hospitals last year and caused infection in others.

The paper was discussed by Drs James Ritchie, Harvey Littlejohn, Dowden, Ronaldson, Lundie, Norman Walker, Webster, and James Carmichael.

Edinburgh Obstetrical Society

THE Sixth Ordinary Meeting of the Society was held on the 9th May. Dr Milne Murray, President, being in the chair.

Several interesting specimens were exhibited, amongst these being an anencephalic fœtus and a bi-partite placenta by Professor Simpson; the bladder and kidneys from a case of suppression of urine by Dr Lamond Lackie. Dr Matheson Cullen showed a specimen of spontaneous amputation of the cervix uteri, with part of the vaginal wall, from a woman suffering from syphilis, and also radiographs showing polydactyly in several members of one family. Dr Foulis demonstrated a new handle which he had adapted to the ordinary axis traction forceps, and Dr Milne Murray brought a beautiful example of a female cyclops infant which had lived for one or two hours after birth.

The adjourned discussion on Dr Haultain's paper on "Pathological Amenorrhœa from other than Constitutional Causes" gave rise to several interesting remarks by various speakers. Dr Haultain, in reply, stated that he looked upon menstruation as a "safety valve" which gets rid of an over accumulation of blood each month. He thought that there must be an extract of a thyroid nature in the ovary which stimulates the blood-forming glands to activity. Those cases of menstruation in anæmic patients, he thought, were due to menorrhagia.

In anæmia the flow was stopped by nature, and such patients ought not, in his opinion, to menstruate.

Professor Simpson read a paper on "Intrauterine Vaporisation," and showed the Vaporiser in action. This paper is published in another part of the Journal.

Dr J. W. Ballantyne read "A Clinical Note on a case of Vulvar Hæmatoma in Labour." This paper is published in full.

Glasgow Medico-Chirurgical Society

AT a meeting on Friday, May 4th, there was shown by Dr Barclay Ness, for Sir William Gairdner, who was unavoidably absent, "a case of probable Addison's disease, associated with tuberculosis and leucoderma." The patient was a female, aged twenty-two years, who had been admitted to the Western Infirmary in January, complaining of cessation of menses and general weakness, of nine months' duration, with periodic attacks of vomiting and pigmentation of the skin. The mother of the patient had died of acute tuberculosis, and of her children 5 out of 11 had died in infancy. The patient was found to be suffering from asthenia; the pulse ranged from 100 to 120; there was an incipient tubercular affection at the left apex; the hæmoglobin was reduced to 58 per cent. The abdominal organs presented nothing abnormal. Browning of the skin and leucoderma were well seen. The latter was present in characteristic patches on the face and forehead (also white tuft of hair), on the arms, hands, fingers, very markedly on the back, and also on lower limbs. Around each nipple there was a leucodermatous patch. The browning was very well marked over the body and limbs, but not on the palms of the hands, or soles of the feet, nor in the mucous membrane of the mouth or lips. The diagnosis lay between Addison's disease and leucoderma (vitiligo). It resembled the latter in some respects, in the nature of the pigmentation, and the unaltered organic condition of the skin. It differed from Addison's disease on account of the white patches which were present. But Sir William Gairdner held that leucoderma may be associated with the pigmentation of Addison's disease. Addison himself had published such a case. Wilks had later declared such not to be true cases of Addison's disease, but the evidence was against that conclusion. Some remarks were quoted which Sir William Gairdner had made on this point, in a discussion at the International Medical Congress of 1881. In the present case Sir W. Gairdner had concluded that it was one of Addison's disease from the

presence of the pigmentary changes, the constitutional symptoms, the asthenia and periodic vomiting, and from the association of the tuberculosis in the lungs. The treatment had included tabloids of supra-renal gland, from 3 to 6 daily. The patient increased in weight to the extent of 7 lbs. The quantity of the urine, and its specific gravity, were increased, from the increase in blood pressure which physiologists had found to ensue on administration of the gland. No further result from treatment was obtained. The progressive phthisical condition of the lung was against success.

Dr Alex. Robertson believed that the diagnosis of Addison's disease was probably justified. The combination of symptoms, and the presence of tubercular disease, pointed to it.

Dr Lindsay Steven had had two cases which had been examined post-mortem. In neither was leucoderma a prominent feature. The palms of the hands had been much pigmented. In the case before the Society it was well worth noting that the mucous membrane of the mouth was not pigmented. The association of active disease in the lungs precluded the necessity, at least, of there being caseous disease in the supra-renal glands. With regard to treatment by supra-renal gland he had seen no benefit from it. From the pathological point of view there was no very obvious parallelism between myxœdema and Addison's disease.

Dr Carslaw said that he had had opportunity of observing the case in hospital, and that with regard to the effects of treatment, it was worthy of note that, despite the progress of disease in the lungs, the patient had felt better, and had increased in weight. There was no improvement, however, in the pigmentation. He had followed another case for more than four years, where the general symptoms and the pigmentation had been very greatly improved—indeed practically cured. There were undoubted cases in which the supra-renal gland had had good effect.

Dr Souttar M'Kendrick referred to an American author who had related the association of leucoderma with Addison's disease without special comment, at which Sir Wm. Gairdner, on its being brought to his notice, was greatly surprised, considering the controversies that had taken place on the subject.

Dr Alex. Robertson wished to add, with reference to the effects on successful treatment by supra-renal gland which the exact pathological state of the patient's glands would have, that he had seen a case of Addison's disease in which it was found, post-mortem, that the glands were affected by simple atrophy. As Dr Ness and Dr Carslaw had remarked, success from gland treatment would be more likely in such cases than

in those in which the glands were the seat of active tubercular disease.

Dr Gunn liked the cautious way in which the billet was stated. It was characteristic, he thought, of Sir Wm. Gairdner, and made for progress. To call a disease simply by a man's name was apt to retard progress. Medical men became anxious to discover a typical case of such a named disease, and to stop there, rather than to investigate each case independently on its own merits.

Dr Ness, in reply, admitted the force of Dr Steven's remarks, but said that the wording of the billet showed that Sir William Gairdner had considered the various points. With regard to the presence of tubercular disease in the lung, and the effect it might have in producing the symptoms, there were undoubted cases of Addison's disease, proved by post-mortem, when also cavities had existed in the lungs. Such a case had been shown to the Society some years ago.

Dr W. K. Hunter showed (*a*) a case of Friedreich's disease. The patient was a boy, aged fifteen years. It was an isolated case, there being nothing hereditary in the family history. He had begun to walk when two years old. Then rickets had interfered with his walking, but after a time he walked well. Recently great difficulty had presented itself. One leg was now distinctly atrophied—the left. Three or four years ago his heels became drawn up. A surgeon had performed tenotomy. His intelligence was good. He had been somewhat deaf, and increasingly so, for the past five years. There were three curvatures in the spine, dorsal, lumbar-compensatory and lateral. Inco-ordination was the most marked feature, seen best in the arms and legs. There was no definite muscular paralysis, nor any reaction of degeneration. Genu Valgum (?) and Pes Cavus were well marked. It was a question whether the Genu Valgum was real (bony) or apparent (muscular). There were no knee jerks, nor ankle clonus. There was some loss of sensation in the legs, more marked in the left than in the right foot. Déjerine had published two cases where there was some muscular atrophy, and some loss of sensation. When first seen the case had suggested three possibilities in diagnosis: (1) infantile paralysis; (2) myopathy, of the Charcot-Marie type; (3) Friedreich's disease. There had been no difficulty in adhering to the last named as the correct diagnosis.

(*b*) A case of pseudo-hypertrophic paralysis. The patient was a boy aged twelve years. His mother had three brothers, all of whom had apparently died from pseudo-hypertrophic paralysis. The features of the case were shown to the Society, and were typical.

The cases were briefly discussed by Drs Gray, Alex. Robertson and Young.

At a meeting on May 11th, Dr Walker Downie showed two men who suffered for years from severe and prolonged attacks of asthma, associated with, and apparently dependent upon, the presence of nasal polypi, extirpation of which has resulted in complete immunity from attacks of asthma.

The first patient was a man, aged forty-one years. He had had asthma, which was worst at night on lying down, and latterly brought on by walking quickly, finally being continuous day and night. On examination the sense of smell was absent in the left naris, and deficient in the right, with blocking of left passage, and partial blocking in right. In the right naris there were numerous small polypi occupying the middle and superior meati. The left naris was wholly occupied by several large mucous polypi. These were removed at intervals by the snare and forceps. With improvement in the nasal condition the asthmatic attacks became less. Later on the polypi returned, as did the asthma. They were again removed, and the tissues from which they sprang cauterised. The asthma was much benefited again. Since March 1897, when the nares were finally and thoroughly cleared, he has not had a single attack of asthma.

The next patient was a man, aged forty-two years. For many years he had had frequent attacks of sore throat, and also a stiffness in his nose—but the organ had never been examined. Five years ago he had first suffered from asthma, and each attack of inflammation of the throat was followed by asthma. On many occasions he was completely exhausted by long attacks, and thought to be dying. There was found by Dr Downie to be difficulty of breathing by the nares, and absence of smell. There was general hypertrophy of the mucous membrane, with several large polypi. These were removed at several sittings, along with the anterior half of the middle turbinated bone, and free searing of the bases of the polypi was effected on various occasions afterwards. There was immediate relief from all attacks of asthma. Four months later he contracted whooping-cough, and he had a slight attack of asthma, but they disappeared on recovery from the whooping-cough. Early in 1900 he again began to wheeze on lying down at night, and again a small mucous polypus was found in the left naris. This was removed, and he obtained complete relief. Dr Downie had treated many cases of asthma associated with nasal mucous polypi and turbinated hypertrophy. The majority of these cases were in women of a somewhat neurotic tendency. The two patients before the Society were, however, vigorous

men. He believed that in the great number of cases of bronchial asthma, complicated with the presence of nasal mucous polypi, that if the asthma is not relieved by the removal of the polypi, it is because the operation has not been completely carried out. About thirty years ago Voltolini first drew attention to the dependence of some cases of bronchial asthma upon nasal lesions, he having cured such a case. His observations were supported by several writers at the time, but the importance of the association between nasal disease and asthma was not generally recognised. In 1896 Bosworth, of New York, stated that in all the cases of asthma he had seen there existed an intra-nasal lesion. In Dr Downie's opinion, despite that to the contrary held by some, there were many cases of pure bronchial asthma in which the exciting cause lay unsuspected within the nares.

Aberdeen Medico-Chirurgical Society

A MEETING of this Society was held on May 3rd, Dr W. H. Williamson, President, in the chair.

The evening was devoted mainly to the exhibition of clinical cases. The following were shown :—Dr Rose—two cases of *tubercular peritonitis* which had been successfully operated on; Dr M'Kerron—a case of *chronic basilar meningitis*; Dr A. W. Mackintosh, for Dr Edmond—a case of *diplegia facialis*; Dr Marnoch for Dr Garden—a case of *osteitis deformans*; Dr Gordon—a case of *acromegaly*; Dr Lister—a case of *Raynaud's disease*; Dr Galloway—a case of penetrating wound of the eyeball, with skiagrams and apparatus. Dr Levack exhibited a series of skiagrams.

Obituary

Nurse Mary S. Boyd, of the Edinburgh S. African Hospital

DEATH has already broken the ranks of the Edinburgh Hospital, which left Edinburgh so recently as the 24th of March.

When the *Briton* arrived at Cape Town it was found that the destination of the Hospital was Norval's Pont, and the vessel accordingly went to Port Elizabeth. The nurses were, however, borrowed for temporary duties until the Hospital should be ready, and were sent up to Naaupoort for duty at the Hospital there. Nurse Boyd must have been infected almost immediately, for three telegrams arriving in rapid succession—the last

on May 17th—announced—the first that she was down with typhoid, the second that her condition was grave, while the third announced her death.

Nurse Boyd had served her full term of training in the Royal, and all last winter was assistant staff in the ear and throat and skin departments, from which she volunteered for duty in South Africa.

The daughter of Mr John Boyd, so long and so pleasantly associated with medical publication in Edinburgh, and the sister of Dr F. D. Boyd, one of the Assistant Physicians to the Royal, and Physician to the Edinburgh South African Hospital, she was a universal favourite with those under whom she worked. Always willing, always pleasant, and exceptionally capable, she was an ideal nurse, one who could be trusted absolutely in the management of any case. We know that more than one of the assistant staff looked forward to having her in charge of their wards.

Fate has decreed otherwise, but it must be some little consolation to her family to feel that in the discharge of her duty she has died for her country as truly as any who fell storming the trenches.

"*Dulce et decorum est pro patria mori.*"

N. W.

New Books and New Editions

On Granular Kidney and Physiological Albuminuria. By SAMUEL WEST, M.A., M.D. Oxon., F.R.C.P. Lond., Assistant Physician, St Bartholomew's Hospital, etc. Demy 8vo, pp. 198. London: Henry J. Glaisher. Price 7s. 6d. net.

THIS volume, while professing to be the Lettsomian Lectures, delivered before the Medical Society of London, contains, we think, a good deal more than the lectures contained, and it is therefore of greater value. It always seems to us to be hardly creditable to clinical medicine or to pathology that there should be so much confusion and variety of opinion regarding the changes in the kidney and the other changes which may be present in the class of diseases known as nephritis. It thus becomes a matter of special interest to find a book dealing with one form of kidney disease, for in it we might find at least one definite position round which others might in due time range themselves. Dr West confines himself to granular kidney, and he takes as the type of this condition the small granular kidney with arterio-sclerotic changes in the renal and other vessels. Here and there, however, throughout his paper he refers to

microscopical characters the same as those of *granular* kidney without the kidney being granular. He criticises recent writings on granular kidney because the *arterio-sclerotic* and the *renal* forms are considered, the one under diseases of the vessels, the other under diseases of the kidney. He says, "this is very inconvenient and confusing, and suggests a marked difference between the two forms which, clinically, does not exist." He at the same time acknowledges that it has the advantage of fixing attention on the question at issue, namely, "whether it is in the vessels or in the kidney itself that the primary causes of the disease are to be sought." These two statements are the key of Dr West's position. He starts from the clinical standpoint of no clinical distinction between two forms, and then states that the question still remains whether the primary cause is in the vessels or in the kidney. The arrangement he criticises is, we take it, the indication that it begins to be perceived that there are arterio-sclerotic changes in vessels which produce kidney changes because renal vessels are implicated, while on the other hand there are primary interstitial kidney changes which lead to certain arterial changes. If these two conditions have a distinct individuality, as we believe they have, it lies with the clinician to discover the clinical difference between them, and he is not to console himself with the presumption that it does not exist. Dr West's argument is based on the existence of granular kidney with arterial disease, and the whole aim of his contention seems to us to be to establish an entity which can be readily recognised clinically. We entirely agree with him up to a certain point, but would suggest that for the satisfactory elucidation of the question he is interested in it is necessary to make an extensive series of observations on the kidneys of all subjects which appear on the *post-mortem* table. By this means a clue might be obtained which would clear up the nature of the early changes in kidney cirrhosis, whether primarily renal or primarily arterial. Much of the error and confusion which prevails regarding the kidney result from confining examinations to cases showing advanced changes when it may be impossible to determine which are primary and which secondary. This Dr West recognises, for he advocates the earlier clinical recognition of the condition; but it is to be borne in mind that even before that is definitely practicable there are already structural changes. Such observations as have been suggested above would show that acute diffuse nephritis might easily be preceded by a diffuse interstitial overgrowth, as Dr West suspects to be present, judging from clinical and *post-mortem* observations; that it is always so is open to question. This, however, is not the place to examine at length or in detail the questions

raised in Dr West's lectures. There is, however, no doubt that they constitute a most valuable and able contribution to the questions discussed, and they will not fail to command attention from all interested in the elucidation of kidney diseases.

The volume contains an important chapter on physiological albuminuria, and we are interested to see that Dr West leans to the view that the kidneys probably have an internal secretion. His observations with pilocarpine and renal extract are certainly very striking.

A Manual of Surgery. By CHARLES STONHAM. F.R.C.S.Eng., Senior Surgeon to the Westminster Hospital, etc. etc. 3 vols. 8vo, pp. 1451, 446 illustrations. London: Macmillan & Co., Limited. Price, Vols. i. and ii., 7s. 6d. net.; Vol. iii., 10s. 6d. net.

THIS is an exceedingly readable manual, and the author is to be congratulated on having succeeded in bringing the essentials of surgery within such comparatively reasonable compass, albeit he, or his publishers, have deemed it advisable to present the work in no fewer than three volumes. This we think was hardly necessary, either on account of the bulk of the material, or from the point of view of the reader's convenience. There may be some justification for sub-dividing a large work into several smaller volumes, if each contains a definite and distinct section of the subject matter, and if it can be clearly indicated on the back of each volume what is to be found therein. In the work before us this is not the case. For example, Vol. i. is titled "General Surgery," but does not contain the chapters on diseases of blood-vessels, aneurism, diseases of bones, joints, muscles, etc. These subjects we have to seek for in Vol. iii., which is labelled "Regional Surgery." "Deformities," on the other hand, are included in general surgery, although they lend themselves better to regional description than some of the above named. Again, we find the chapter on "Antiseptic Surgery" in Vol. ii., which is devoted to "Injuries," and in spite of this fact, in the first volume many operative methods of treatment have been recommended, and some described, before ever the first principles of the antiseptic method have been discussed. Even with regard to injuries, however, we do not find them all in Vol. ii., as we have injuries of the ear treated of alongside the diseases of that organ in the third volume. We may here say that we see no advantage to the student in describing the diseases of a region or organ apart from its injuries, nor is this separation at all essential to the systematic writer.

The work is profusely illustrated, and although the great majority of the figures are borrowed, and not a few have already become almost hackneyed by much copying, they are none the less instructive. The paper, type, and binding leave nothing to be desired. So much for the manner of the book.

When we come to the matter we find it, on the whole, of a high standard of excellence. It is well and carefully written, and only here and there does the author's English become a little loose, as in the paragraphs on the "causes of sinus" (Vol. i., p. 52), and on the "etiology of tubercle" (Vol. i., p. 144). If we were to select any chapters in the first volume as specially useful to the student, perhaps they would be those on surgical bacteriology and on deformities. The opening chapter on degenerations, however, might easily have been dispensed with. It is perfectly true to say that "in all cases of renal inadequacy morphia is a particularly dangerous drug," but why should this isolated statement come in under the heading of "fatty degeneration"? The author's views on inflammation are thoroughly orthodox, in spite of which his description of the pathological processes and clinical features associated with it is excellent.

The second volume (on Injuries) does not appear to be so uniformly good as its neighbours. On the whole, it is not so well up to date, for example, in such matters as antiseptic agents, the true *aseptic* method of operating—which, by the way, we would point out to the author, is practised elsewhere than on the Continent—the treatment of bruises, burns, and fractures. This volume, however, contains an exceedingly useful chapter on the Injuries of the Eye, from the pen of Mr Donald Gunn.

"Regional Surgery," properly so-called, being all dealt with in the last 500 pages of the third volume, receives less than its due share of attention; but, so far as it goes, it is a faithful and useful presentment of the subject.

Difficult Digestion due to Displacement. By A. Symons Eccles, M.B. Demy 8vo, pp. 138, with 27 illustrations. London: Baillière, Tindall & Cox. Price 4s.

The subject with which this unambitious monograph deals is not only of great interest, but knowledge of it cannot be regarded as general in the ranks of the profession. Unfortunately the subject is only treated of from the purely clinical standpoint; there is a want of *post-mortem* checks on the author's observations and contentions, and this deficiency

necessarily reduces the value of these. The kind of accuracy and precision desirable in treating a subject which involves displacements of viscera can only be attained by considerable experience in the *post-mortem* room, and we do not gather from the pages under review that the writer of them has had such an experience. While we hold that the experience we have indicated is necessary for the satisfactory exposition of the subject dealt with, Dr Eccles has performed a valuable service in bringing the subject forward. The introduction deals in a general way with the subject-title of the book and the treatment the author applies; this is followed by four chapters which deal respectively with gastroptosis, movable kidney, general enteroptosis, and prolapse of the sigmoid flexure. With reference to the first of these the question might fairly be asked, whether some at least of the cases were not gastrectasia rather than gastroptosis: the author distinguishes between the two, but we are inclined to be sceptical as to the reliability of his anatomical land-marks. No doubt an atonicity of the body of the stomach comes clinically to much the same thing as a moderate gastroptosis, but then in marked gastroptosis the position of the pylorus is also much altered, and the treatment is then a much more difficult matter. The chapter on prolapse of the sigmoid flexure will be to many the most novel, and it like the others is illustrated by many cases.

The outstanding value of this monograph is, we venture to think, the insistence upon the practicability of diagnosing the position of the hollow viscera, and their efficient treatment without the aid of the surgeon. The author further realises the determining part played by displacements in producing neurasthenia, debility, and conditions of general ill-health, all of which form a department of practical medicine on which there is much room for increased recognition. We cordially commend the work as a valuable and practical record of good clinical work.

An Introduction to Diseases of the Nervous System. By H. CAMPBELL THOMSON, M.D. (Lond.), M.R.C.P., Physician to Out-Patients to the Hospital for Epilepsy and Paralysis, Regent's Park, etc. Demy 8vo, pp. 124, with 25 illustrations. London: Baillière, Tindall & Cox. Price 4s.

THIS is an admirable little book. As the author states in the preface, it is an introduction to the study of diseases of the nervous system. The line taken is to sketch the groundwork of the subject in such a manner as the author has found from experience in teaching to be useful to students. In this he has certainly succeeded. The groundwork of the subject is clearly

and succinctly laid out, and the book will be found of assistance to the student in understanding and in carrying out the methods of investigation in the important class of cases dealt with.

Recent Literature

CRITICAL SUMMARIES AND ABSTRACTS

MEDICINE

ORGANOTHERAPY

By G. LOVELL GULLAND, M.A., B.Sc., M.D., F.R.C.P.Ed.,

Assistant Physician to the Royal Infirmary; Physician to the Victoria Hospital for Consumption, Edinburgh, etc.

ON this subject there have appeared within recent years more papers than it would be possible to quote without unduly overloading this summary with names. Many of them will be found in "L'Opothérapie," by Gilbert and Carnot, Paris, 1898, and there is a useful collation in Richter's "Die Organotherapie und ihre praktische Bedeutung," *Samml. Klin. Vorträge der Berliner Klinik*, Heft 139, 1900, to which I am much indebted.

The whole idea of this mode of treatment of disease depends upon the theory of "internal secretion"; the belief, first formulated by Brown-Séquard, that certain organs, not all of which are necessarily glandular in the ordinary sense, produce materials which pass into the blood, and through it influence all the cells of the body, so that the loss of the secreting organs or the cessation of their secretion is injurious to the whole organism. The purpose of organotherapy, therefore, is to supply to the organism something which it has lost, and the loss of which has produced morbid phenomena; it is a method of substitution. To give organic extracts in cases of functional perturbation of the respective organs where there is no question of the loss of an internal secretion, or where in the nature of things it is impossible for an internal secretion to be formed, is quite another matter. Organic extracts in such cases may do good or may do harm; the use of "cerebrin," "medullin," etc., in nervous conditions may result in good by suggestion, and similar preparations have been employed by Chinese physicians for many centuries, presumably with the same result as at present.

But for organotherapy in the proper sense, it is necessary to know first that the affected organ produces an internal secretion, as is now known in the case of the thyroid gland, for example, both from experiment and clinical observation; and second, that

in the disease which is to be treated this internal secretion is in abeyance, and that it is capable of replacement. That is to say, for rational treatment an exact knowledge of pathology is required.

It is from the study of the **thyroid gland** and its diseases, indeed, that we have the foundation of all our knowledge of internal secretion and organotherapy, and the subject is too fresh in our minds to require repetition here. It is interesting to note, however, how the exhibition of the remedy has advanced along lines which will probably have to be followed in the case of other organs. First, there was the implantation of thyroid substance into some part of the body, a difficult process and of uncertain result. Then followed the hypodermic injection of glycerin extracts, the ingestion of raw or cooked thyroids from animals, and later of dried thyroid substance in tabloid form. Baumann's discovery of an organic iodine combination in the thyroid gland, though of great scientific importance, is not a great practical advance, for the dried whole gland is quite as useful as "iodothyrin."

The diseases which have been treated with thyroid substance are innumerable, but in a few only has success justified the treatment. The first of these is, of course, **myxœdema,** in which a carefully controlled use of the remedy will always do good. Almost equally useful is it in **sporadic cretinism** and **infantile myxœdema,** though here the unfortunate reservation has to be made that the child's intellect does not as a rule keep pace with his improved bodily development, though the psychical disturbances of myxœdema disappear in the course of treatment. Another disease, comparatively rare in this country, but very common abroad, in which thyroid feeding is undoubtedly useful, is **endemic goître,** especially the follicular hyperplastic form. Here, particularly in young subjects, the swelling diminishes greatly, though it rarely disappears entirely, and the gland resumes its normal histological appearance.

In these conditions we have a real treatment by substitution. But there are other diseases where thyroid has been used mainly because of some of the by-effects which have been observed in the treatment of the strumiprivous conditions. Thus, for instance, when the gland is removed in young animals the growth of bone and cartilage is slowed and fractures heal badly, and it is not unnatural therefore to experiment with thyroid feeding in certain **ununited fractures,** and in other **diseases of bones.** It is carrying empiricism to its limits, however, to treat the most varied kinds of **mental conditions** with thyroid, merely because myxœdema produces mental symptoms which can be removed in that way. In some of these

cases there is undoubtedly a good effect, but one knows of others where harm has been done. Speaking generally, the conditions where it has been useful are those associated with mental depression. In the same sort of way the remedy has been tried in all sorts of skin diseases, because it was found that the harsh and dry skin in myxœdema became soft and natural under its influence. **Psoriasis** and **ichthyosis** are the only diseases where it has been found really to be of use, and I have always thought that it was a good deal better for the patient to retain his psoriasis than to submit to the emaciation, and to the cardiac and gastric disturbance, which are inevitable before the disease is removed. For we have to remember that thyroid substance in excess really acts as a poison, and nowhere has the want of recognition of this fact led to more unpleasant results than in the treatment of cases of **exophthalmic goître**. A moment's consideration of the state of the thyroid gland in that disease shows us that it is overacting, not underacting, and a comparison of the symptoms of an acute case with those of a patient suffering from thyroidism shows us that the two conditions are practically identical. In both cases the body is being flooded with thyroid secretion, and it is not only useless but dangerous to give more of it.

Another property of thyroid substance has been made use of, its power of increasing metabolism. The earliest observers noted the great decrease in weight in myxœdematous patients under treatment, and very soon the substance was used as a means of reducing weight, especially in **obesity**. It achieved a rapid popularity among the obese, because it was found to reduce weight without requiring restriction in diet, which such patients always object to. It was at first thought that this was the result of a true substitution. Clinically and experimentally two kinds of obesity can be distinguished—one, namely, where the condition results from over-feeding along with deficient bodily exercise; the other, in which, as a result of a constitutional anomaly, a slowing of metabolism, the patient puts on fat, even with a low diet, because the oxidative processes in the cell, which should consume the fat, are deficient. It was supposed that the latter form only was influenced by thyroid, which was assumed to increase oxidation, and so to lead to the burning up of the fat. It has been found, however, that not only both classes of adipose patients, but also ordinary healthy people, are reduced by thyroid in the same way, and that this property is to a certain extent a poisonous one, for the oxidation of the non-nitrogenous materials is accompanied by a well-marked loss of proteid. This evil effect, however, can, to a certain extent, be prevented by a full diet, rich in proteid, provided the treatment is not continued for too long a time. If it is too long

persisted in the body is greatly weakened, and it is best to use the remedy for a short time at the beginning of treatment, and to follow it by the proper regulation of diet, exercise, etc.

We must not forget that thyroid substance may produce unpleasant symptoms not only when the dose is too great or the treatment too long continued, but even when due care is taken, and this makes constant medical supervision necessary during its use. The most common of these symptoms are insomnia, headache, palpitation, giddiness, and loss of appetite. It is well to know that arsenic given simultaneously up to a dose of twelve drops in the day often prevents or removes these symptoms.

A few years ago a great deal was expected from the **pancreas** in the treatment of **diabetes.** It was discovered that its experimental extirpation was followed by severe diabetes, though what the real relation of the gland to the utilisation of sugar is, has not yet been made out. It seems obvious, however, that the pancreas does yield an internal secretion, and the re-implantation of the excised gland in some other situation has been found to prevent the glycosuria, just as the implantation of an excised thyroid prevents the occurrence of the cachexia strumipriva. Unfortunately the analogy carries us no further. Pancreatic substance has been given to diabetics in every possible form and way, but hitherto without success. It is, of course, possible that the preparations were not active, or that the active principle was destroyed before absorption, perhaps by the acids of the stomach, for instance.

Preparations of the **testicle** and **ovary** have been used in diabetes, and have diminished the excretion of sugar both during and after their use. This is, of course, not due to any special relation between these glands and the glycogenic function, but to an increase of the oxidative processes in the body, something like that caused by thyroid. It is known from other sources that ovarian substance certainly possesses this power. This would bring about a temporary increase in the power of the body-cells to utilise sugar, so that less would appear in the urine.

The **pituitary gland** is another which has disappointed expectation. Its increase in size in **acromegaly** led to its use as an internal remedy in that condition, but it has since been found that acromegaly is more akin to exophthalmic goître than to myxœdema or endemic goître, and that it is an active hypertrophy of the gland which produces the overgrowth of certain parts of the body. To give pituitary substance in this disease is, therefore, as ill-judged as to give thyroid substance in Grave's disease, and just as useless.

The **suprarenal capsules** have yielded something of value in treatment, though not in the expected direction, for in **Addison's disease**, which results from their destruction, it has been found quite useless to give their substance. There is more promise in the experiments which have been made with suprarenal extract in connection with blood-pressure. The removal of the organs causes a fall in blood-pressure, and their extract when administered internally causes it to rise, and it is quite possible that it may be of use as a tonic in conditions of cardiac debility. The same extract has the very curious property of causing total anæmia of the conjunctiva when applied locally, and has already been found useful in ophthalmic surgery. The active substance in the suprarenals has been isolated and found to be a definite chemical substance, allied to brenzcatechin, and it is possible that this substance is responsible for the good effect produced by suprarenal preparations in **rickets**, either from a specific effect on the growth of bone, or by its general tonic properties.

Extracts from the **spleen** and **bone-marrow** have been found experimentally to increase the number of leucocytes in the blood, probably because of the amount of nuclein which they contain, and also to increase the number of red blood-corpuscles. In practice, however, very little has come of their use in pernicious anæmia, leukæmia or chlorosis, and nothing is really to be expected in the way of substitution, for in the two former diseases the bone-marrow is not only not inactive, but is working double tides, whilst in chlorosis there is simply a functional lassitude of the bone-marrow, which does not seem to be affected by marrow preparations. Of the relations of the spleen to blood diseases, we know so little that it is the wildest empiricism to use splenic extract in such conditions at all.

The use of kidney extract in uræmia, lymphatic gland extract in scrofula, lung extract in phthisis, of "cardin" and "hepatin" in diseases of the heart and liver, need only be mentioned in order to be reprobated, and all that is left of Brown-Séquard's claims for testicular juice is the fact that it has tonic properties, and, like other organic extracts, produces leucocytosis.

Ovarian substance, on the other hand, seems destined to have a wide range of usefulness. Everyone knows the symptoms which often accompany the physiological loss of the ovaries at the menopause, or their loss by operation. These symptoms are mainly vasomotor in character, and, without being dangerous, are often sufficiently annoying, and there is no doubt whatever that ovarian substance given internally is capable of removing or preventing them in the great majority

of cases. It seems that we have here to do with a real substitution. The influence of the ovaries on the economy of the female is so great that it is difficult to doubt that they produce an "internal secretion," and when the organs are lost they can be replaced, though only in this respect, by artificial means. If it be objected that the symptoms to be relieved are mainly nervous, and capable of being influenced by "suggestion," it may be pointed out that there is another symptom associated with loss of the ovaries, whether experimental, veterinary, operative, or menopausal, namely, obesity. This is due, in this case, to a slowing of metabolism which results from a diminution of the oxidative energy of the cells of the body. There must therefore be some substance in the ovary which regulates the course of oxidation in the body, and if ovarian substance can be used for a real treatment by substitution, it should have the power of stimulating metabolism where the ovaries are absent, and of leaving it unaffected in persons with ovaries which are exercising their normal functions. This is found actually to be the case, and so far as experiment and clinical observation have gone, ovarian substance seems to be the best remedy for menopausal obesity. It has, moreover, the immense advantage over thyroid substance, which has a similar effect on metabolism, that it is entirely free from toxicity, and does not break down proteids. This enables the remedy to be used for long periods and in large doses.

Chlorosis is by some regarded as due to loss of an ovarian secretion, which is important for blood formation, but the experiments in this direction are as yet too incomplete to allow one to draw any conclusions.

From what has been said it will be seen that often two or more organs seem to have very similar properties, and there is in all probability a wide field of work in determining the nature of these relations. Another important step which will be made, is the determination of the chemical nature of the active principles of different organs, and the exact testing of their physiological and therapeutic action.

GYNECOLOGY AND OBSTETRICS

By A. H. F. BARBOUR, M.D., F.R.C.P.Ed.,

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Has Posture a Material Influence on the Form and Dimensions of the Pelvis?—This is a question which has been much canvassed recently in French and German obstetrical literature. This inquiry recalls Matthews Duncan's paper on

"The Pelvic Joints in Parturition" in the *Dublin Quarterly Journal for Medical Science* for 1854. Duncan's paper was based on the observations of Zaglas, published in the *Monthly Journal for Medical Science*, 1851, which showed that in the human female there is a movement of rotation of the sacrum relatively to the innominate bones round an imaginary axis passing transversely through the second sacral vertebra.

The obstetrical significance of this "mutation" of the sacrum is thus described by Duncan. "In the course of the first stage of labour, while the head is pressing into the brim, the human female is generally standing, sitting, or lying on her back, or in an easy position. But as soon as the head has descended into the pelvis and infringed upon the sensitive vagina, then forcing efforts accompany the pains. These forcing efforts consist, in great part, of powerful contractions of the anterior abdominal muscles, the effect of which, especially the action of the two recti muscles, will be to tilt up the symphysis pubis, thus throwing the promontory forwards, contracting the brim, and enlarging the outlet, and diminishing the angle of inclination of the outlet. . . . And it is a curious fact, that a woman in her forcing pains, in the second stage, is found to draw up her legs, and bend her body forwards, thus inducing changes in her pelvis which facilitate the advance of the child in that stage." He says further, "that the alterations in the dimensions of the brim and outlet by these movements is not insignificant, but the reverse, is a proposition which every obstetrician will confirm." In a footnote to this paper, as published in the "Mechanism of Natural and Morbid Parturition," he adds, "another motion is described by Laborie, namely, separation of the ischiatic tuberosities. But it appears to me to demand further inquiry before being accepted as true." We draw attention to these statements of Duncan because they have not received sufficient recognition in the more recent discussion of this subject. It should be noticed, however, that though he alludes to possible changes in the size of the brim, the advantage of the movement for labour, according to Matthews Duncan, is at the outlet only.

In 1881 Crouzat drew attention to the greater mobility of the pelvic joints in pregnancy as being beneficial for labour, and in the same year, Korsch showed from experiments on the cadaver that the antero-posterior diameter of the brim could be distinctly increased in length, the transverse less markedly; while the effect of these changes at the brim was to produce contraction at the outlet.

It was, however, Walcher's paper in 1889 that brought the question into the field of practical obstetrics, and provoked the discussion mentioned above. He noted that by bringing the

patient to the edge of the couch and placing a pillow under the sacrum so as to raise it high enough for the legs to hang free, there was distinct increase in the length of the diagonal conjugate as measured by the finger. The increase varied from ·3 to ·5 in. (8-13 mm.). The true conjugate would not of course be increased so much, but if anything like this amount of room could be gained in the length of the conjugate of a contracted pelvis by merely placing the patient in this posture, which is now known as Walcher's position, it would prove a great gain for the management of such cases.

These observations of Walcher on the living subject led to a number of experiments on the cadaver, on which more exact measurements can be made. The first important series of these was made by Klein in 1891, who experimented on the pelvis with weights on a pulley to reproduce the traction exerted by the lower limbs. He found that the increase in the conjugate that could be obtained under these conditions was much less than Walcher described, the greatest being ·4 in. and the average much less. Klein's observations were made on 47 pelves, 17 male and 30 female, of which none were puerperal.[1]

The most important recent papers on this subject have been those read at the Amsterdam Congress, and published in the *Annales de Gynécologie* for August and September of last year.

Lebedeff and Bartoszewicz gave the results of measurements of 25 cadavera, of which 9 were from nulliparæ, 2 from multiparæ, and 5 puerperal. These last were taken from the third to the tenth day of the puerperium. The measurements were made with Klein's instrument, and in three positions—the horizontal, Walcher's, and with the knees drawn up on the abdomen-hyperflexion. They found an increase of the conjugate in Walcher's position as compared with the horizontal not exceeding ·12 in., and that the effect of hyperflexion is to diminish the conjugate, the diminution passing from the horizontal to this latter position being from ·04 to ·28 in. (1-7 mm.); that is to say, that while by drawing the knees up on the abdomen the conjugate may be shortened by ·2 in., by letting the legs hang free it can only be lengthened by ·1 in. Further, they found that the variations at the outlet are the reverse and more marked, its antero-posterior diameter being increased by hyperflexion up to ·4 in. There was no difference between the pelves from nulliparous and puerperal cases.

In the same journal a paper is given by Bonnaire and Bué,

[1] The term puerperal in this discussion includes both pregnant and recently delivered women, as the condition of the joints is supposed to be similar in both these states.

giving the results of the examination of 12 cadavera, from the later months of pregnancy or recently delivered. The viscera were removed, the promontory and pubis exposed, and the soft parts of the perineum removed, so as to make the inner aspects of the ischial tuberosities accessible for measurement. The measurements were made with slips of willow of different lengths inserted between the bony points, and for three postures—the horizontal, Walcher's, and with the knees approximated to the shoulders. In six of the cases the transverse of the outlet was measured, as well as the conjugate of the brim. They found for Walcher's position an increase in the length of the conjugate of from ·04 to ·28 in.—an average of ·14 in. The increase in the transverse of the outlet produced by the posture of hyperflexion was much more marked. The tuberosities became separated ·64-·72 in. in this posture, as compared with Walcher's position. The latter position is thus bad for the passage of the head through the outlet, while the crouching position is distinctly favourable. Their paper therefore gives proof of a movement of the innominate bones in addition to the movement of the sacrum which Walcher drew attention to. It is the same movement as Laborie referred to, and might be described as a rotation of the innominate bones round an antero-posterior axis, whereby they converge slightly at the brim and separate more distinctly at the outlet, producing an increase in the transverse diameter.

Another contribution was made on the same subject by Pinzani, giving the result of measurements made clinically on 102 patients, and post-mortem on 5 cadavera. He found the increase in passing from the position of flexion to Walcher's to average ·3 in., but that the difference in passing from the position of flexion to the obstetrical one is not so great as in passing from the obstetrical to Walcher's. Measurements on the cadaver show the same result, but to a less degree than on the living subject. The alteration in the antero-posterior diameter of the outlet is the converse of the change at the brim and is also more marked.

The papers by Walcher and Varnier on the same subject are polemical without adducing fresh observations. Walcher defends his first statements which Varnier criticises, disputing especially the originality of Walcher's observations, and maintaining that posture is of no value apart from symphysiotomy.

More observations are needed with regard to the effect of posture on the transverse diameter of the outlet, but meantime we may gather up the results of this discussion as far as it concerns the practitioner. Marked flexion of the thighs on the abdomen diminishes distinctly the length of the conjugate of

the brim, but increases the antero-posterior diameter and probably also the transverse diameter of the outlet. On the other hand, by allowing the knees to hang free over the side of the couch we increase slightly the conjugate of the brim, and probably diminish the transverse of the outlet. In applying forceps for a contracted pelvis, the first posture is less favourable for the passage of the head through the brim, while the latter posture, though unpleasant for the patient unless she be anæsthetised, is more favourable. Once the head has passed the brim, or if the contraction be in the lower part of the pelvis, extreme flexion of the thighs increases the available space.

DISEASES OF THE EAR, THROAT AND NOSE

By R. M'KENZIE JOHNSTON, M.D., F.R.C.S.Ed.,
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The Silver salts in suppuration of the Middle Ear.—One of the oldest remedies in the treatment of aural diseases is nitrate of silver. When used in solid form it is a feeble cauterant to the mucous membrane, producing a superficial slough. When used in strong solution it acts as an irritant and produces little sloughing, while in weak solution it acts as a stimulating astringent. It also has a mild antiseptic action. A few years ago the application of solid nitrate of silver to the stump of a polypus was a very usual method of treatment, but of recent years the more powerful chromic acid has entirely taken its place. In chronic otorrhœa, the use of its solution has not proved very successful, and alcohol or powdered boric acid has proved less irritating and more beneficial. Gleason points out that two organic silver salts—**Argonin and Protargol**—are said to be astringent without being in any way irritating to the mucous membrane. He found argonin to be unstable in solution, but protargol, in his hands, has given excellent results. He records four cases of protracted attic suppuration treated by him with this drug. He used a five per cent. solution, which he injected by means of a Blake's cannula as high into the attic as possible. The parts were then massaged with Siegle's speculum, in order to force the solution more deeply if possible. All the cases did well in a remarkably short while, and it would appear that we may have in this silver salt a drug of considerable value in diseases of the ear (*Laryngoscope*, vol. viii., No. 3).

The Principles of Stuttering (Therapeutics).—The theory that the habit of stuttering should be considered as the external expression of a lowered air-pressure of the lungs, or in other

words, as a diminution of the faculty of breathing, was advocated by R. Coen (Vienna) as long ago as 1872. However this may be, it is generally acknowledged now that "breathing gymnastics" based on this theory are an indispensable factor in the cure of this class of case. In a recent paper Coen formulates certain principles which are the result of twenty-five years' experience of the treatment of defects of speech. Methodical continued breathing-gymnastics are a prominent and essential part of the treatment. In carrying out these exercises, which should always precede any voice exercises, the patient should be in the upright position and with his chest movements unimpeded by clothing. The patient's chief efforts should be concentrated on producing: 1. deep continued inspirations; 2. short expiratory movements of the breath; 3. gradual prolonged expirations; 4. holding the breath. The second principle is the regulation of the vocal and speech-producing organs, which consists in enumerating slowly the vowels and diphthongs, gradually increasing the intensity and rapidity of their vocalisation. Then syllables, words, and phrases must be repeated, pronounced loudly and with strong accentuation. Thirdly by means of hydrotherapeutics, electricity, and suitable drugs, we should try to eliminate the spasmodic periods which occasion the disturbances of innovation, diminish the nervous excitability, and increase the activity of the muscles of speech. The fourth principle keeps in view the exercise and strengthening of the will power of the patient. Lastly, it is necessary to bring about a general stimulation and toning up of the system, for it is a general observation that most stutterers are physically weak, anæmic, and nervous individuals.

Along with these principles it should be our endeavour to carefully study each individual case in every detail, and the secret of success will be found in the energy and persistency of their application (*Laryngoscope*, vol. viii., No. 2).

Taking cold.—Personal experience has taught most people the unpleasant effects of taking cold, but the mechanism of "taking cold" is far from clear. It is an erroneous idea to suppose that the condition is the result of the direct loss of heat due to the general exposure to a low temperature. The loss of body heat incurred in great changes of temperature, such as bathing in cold water, etc., is rapidly made up by increased heat production in the system. Clinical experience teaches us that the penalties of exposure are incurred, not from subjecting the whole body to a lower temperature, but when only a portion of it is so exposed. The heat production is regulated by the central nervous system; and if its production is interfered with

in some part of the body, it goes out with increased activity to another part, thus tending to cause inflammatory action elsewhere, usually in the part that has least power of resistance. Thus it may show itself in an attack of rheumatism, lumbago, lung trouble, or very frequently in acute coryza. In discussing the theory of "taking cold," Milbury (Brooklyn) points out that it is often possible to abort the attack if active measures are taken sufficiently early. He first endeavours to restore the equilibrium of animal heat by giving hot drinks, hot foot-bath, etc. In addition he relies very largely on two drugs, quinine and opium. He gives 15 grs. of quinine along with 10 grs. of Dover's powder. These are old friends, but it is a question whether in the present day they are not too much neglected for more fashionable remedies. As a laxative he prefers calomel administered in doses of $\frac{1}{10}$ gr. every 15 minutes until ten doses have been taken. For coryza he finds nothing answer better than atropia, $\frac{1}{1000}$ gr. every 15 minutes until six doses have been taken, and then continued at longer intervals till the throat becomes dry (*Laryngoscope*, vol. viii., No. 2).

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